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THE WORKS

OF

BENJAMIN FRANKLIN.

VOL. VI.

THE

WORKS

OF

BENJAMIN FRANKLIN;

CONTAINING

SEVERAL POLITICAL AND HISTORICAL TRACTS

NOT INCLUDED IN ANY FORMER EDITION,

AND

MANY LETTERS OFFICIAL AND PRIVATE

NOT HITHERTO PUBLISHED;

WITH

NOTES

AND

A LIFE OF THE AUTHOR.

By JARED SPARKS.

VOLUME VI

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PHILOSOPHICAL SUBJECTS.

CAUSES OF EARTHQUAKES.*

FROM THE PENNSYLVANIA GAZETTE, DECEMBER 15TH, 1737.

THE late earthquake felt here, and probably in all the neighbouring provinces, having made many people desirous to know what may be the natural cause of such violent concussions, we shall endeavour to gratify their curiosity, by giving them the various opinions of the learned on that head.

Here naturalists are divided. Some ascribe them to water, others to fire, and others to air; and all of them with some appearance of reason. To conceive which, it is to be observed, that the earth everywhere abounds in huge subterraneous caverns, veins, and canals, particularly about the roots of mountains; that of these cavities, veins, &c., some are full of water, whence are composed gulfs, abysses, springs, rivulets; and others full of exhalations; and that some parts of the earth are replete with nitre, sulphur, bitumen, vitriol, &c. This premised,

1. The earth itself may sometimes be the cause of its own shaking; when, the roots or basis of some large

* This paper is contained in Duane's edition of the author's writings, but in no previous collection. It is taken from the newspaper published by Franklin; but it is dated several years earlier than any of his other pieces on philosophical subjects, and appears to be rather a compilation from various authors than an original composition. It is not without interest, however, as presenting a curious account of earthquakes, and of the theories respecting their causes. — EDITOR.

mass being dissolved, or worn away by a fluid underneath, it sinks into the same, and, with its weight, occasions a tremor of the adjacent parts, produces a noise, and frequently an inundation of water.

2. The subterraneous waters may occasion earthquakes by their overflowing, cutting out new courses, &c. Add, that the water, being heated and rarefied by the subterraneous fires, may emit fumes, blasts, &c., which by their action, either on the water or immediately on the earth itself, may occasion great succussions.

3. The air may be the cause of earthquakes; for, the air being a collection of fumes and vapors raised from the earth and water, if it be pent up in too narrow viscera of the earth, the subterraneous or its own native heat rarefying and expanding it, the force, wherewith it endeavours to escape, may shake the earth; hence there arise divers species of earthquakes, according to the different position, quantity, &c., of the imprisoned *aura*.

Lastly, fire is a principal cause of earthquakes; both as it produces the aforesaid subterraneous *aura* or vapors; and as this *aura*, or spirit, from the different matter and composition whereof arise sulphur, bitumen, and other inflammable matters, takes fire, either from other fire it meets withal, or from its collision against hard bodies, or its intermixture with other fluids; by which means bursting out into a greater compass, the place becomes too narrow for it; so that, pressing against it on all sides, the adjoining parts are shaken; till, having made itself a passage, it spends itself in a volcano, or burning mountain.

But to come nearer to the point. Dr. Lister is of opinion, that the material cause of thunder, lightning, and earthquakes, is one and the same, viz. the inflammable breath of the pyrites, which is a substantial sulphur, and takes fire of itself.

The difference between these three terrible phenomena, he takes only to consist in this; that this sulphur, in the former, is fired in the air, and in the latter under ground. Which is a notion that Pliny had, long before him; "*Quid enim,*" says he, "*aliud est in terrâ tremor, quam in nube tonitru?*"

This he thinks abundantly indicated by the same sulphurous smell being found in any thing burnt with lightning, and in the waters, &c. cast up in earthquakes, and even in the air before and after them.

Add, that they agree in the manner of the noise, which is carried on, as in a train fired; the one, rolling and rattling through the air, takes fire as the vapors chance to drive; as the other fired under ground, in like manner, moves with a desultory noise.

Thunder, which is the effect of the trembling of the air, caused by the same vapors dispersed through it, has force enough to shake our houses; and why there may not be thunder and lightning under ground, in some vast repositories there, I see no reason; especially if we reflect, that the matter which composes the noisy vapor above us is in much larger quantities under ground.

That the earth abounds in cavities, everybody allows; and that these subterraneous cavities are, at certain times and in certain seasons, full of inflammable vapors, the damps in mines sufficiently witness, which fired do every thing as in an earthquake, save in a lesser degree.

Add, that the pyrites alone, of all the known minerals, yields this inflammable vapor, is highly probable; for that no mineral or ore, whatsoever, is sulphurous, but as it is wholly, or in part, a pyrites; and that there is but one species of brimstone, which the pyrites naturally and only yields. The *sulphur vive*, or natural

brimstone, which is found in and about the burning mountains, is certainly the effects of sublimation; and those great quantities of it, said to be found about the skirts of volcanoes, is only an argument of the long duration and vehemence of those fires. Possibly the pyrites of the volcanoes, or burning mountains, may be more sulphurous than ours; and indeed it is plain, that some of ours in England are very lean, and hold but little sulphur; others again, very much; which may be one reason why England is so little troubled with earthquakes, and Italy, and almost all round the Mediterranean Sea, so very much; though another reason is, the paucity of pyrites in England.

Comparing our earthquakes, thunder, and lightning with theirs, it is observed, that there it lightens almost daily, especially in summer-time, here seldom; there thunder and lightning is of long duration, here it is soon over; there the earthquakes are frequent, long, and terrible, with many paroxysms in a day, and that for many days; here very short, a few minutes, and scarce perceptible. To this purpose the subterraneous caverns in England are small and few compared to the vast vaults in those parts of the world; which is evident from the sudden disappearance of whole mountains and islands.

Dr. Woodward gives us another theory of earthquakes. He endeavours to show, that the subterraneous heat, or fire, (which is continually elevating water out of the abyss, to furnish the earth with rain, dew, springs, and rivers,) being stopped in any part of the earth, and so diverted from its ordinary course, by some accidental glut or obstruction in the pores or passages through which it used to ascend to the surface, becomes, by such means, preternaturally assembled in a greater quantity than usual into one place, and there-

fore causeth a great rarefaction and intumescence of the water of the abyss; putting it into great commotions and disorders, and at the same time making the like effort on the earth; which being expanded upon the face of the abyss, occasions that agitation and concussion we call an earthquake.

This effort in some earthquakes, he observes, is so vehement, that it splits and tears the earth, making cracks and chasms in it some miles in length, which open at the instant of the shock, and close again in the intervals betwixt them; nay, it is sometimes so violent, that it forces the superincumbent strata, breaks them all throughout, and thereby perfectly undermines and ruins the foundation of them; so that, these failing, the whole tract, as soon as the shock is over, sinks down into the abyss, and is swallowed up by it; the water thereof immediately rising up and forming a lake in the place where the said tract before was. That, this effort being made in all directions indifferently, the fire, dilating and expanding on all hands, and endeavouring to get room and make its way through all obstacles, falls as foul on the waters of the abyss beneath, as on the earth above, forcing it forth, which way soever it can find vent or passage, as well through its ordinary exits, wells, springs, and the outlets of rivers, as through the chasms then newly opened; through the *camini* or spiracles of Ætna, or other neighbouring volcanoes; and those hiatuses at the bottom of the sea, whereby the abyss below opens into it and communicates with it. That, as the water resident in the abyss is, in all parts of it, stored with a considerable quantity of heat, and more especially in those where those extraordinary aggregations of this fire happen, so likewise is the water which is thus forced out of it; insomuch that, when thrown forth and mixed with the waters of wells or

springs of rivers and the sea, it renders them very sensibly hot.

He adds, that, though the abyss be liable to those commotions in all parts, yet the effects are nowhere very remarkable except in those countries, which are mountainous, and consequently stony or cavernous underneath; and especially where the disposition of the strata is such, that those caverns open into the abyss, and so freely admit and entertain the fire, which assembling therein, is the cause of the shock; it naturally steering its course that way where it finds the readiest reception, which is towards those caverns. Besides that those parts of the earth which abound with strata of stone or marble, making the strongest opposition to this effort, are the most furiously shattered, and suffer much more by it, than those which consist of gravel, sand, and the like laxer matter, which more easily give way, and make not so great resistance. But, above all, those countries which yield great store of sulphur and nitre, are by far the most injured by earthquakes; those minerals constituting in the earth a kind of natural gunpowder, which, taking fire upon this assemblage and approach of it, occasions that murmuring noise, that subterraneous thunder, which is heard rumbling in the bowels of the earth during earthquakes, and, by the assistance of its explosive power, renders the shock much greater, so as sometimes to make miserable havoc and destruction.

And it is for this reason, that Italy, Sicily, Anatolia, and some parts of Greece have been so long and often alarmed and harassed by earthquakes; these countries being all mountainous and cavernous, abounding with stone and marble, and affording sulphur and nitre in great plenty.

Further, that Ætna, Vesuvius, Hecla, and the other

volcanoes are only so many spiracles, serving for the discharge of this subterraneous fire, when it is thus preternaturally assembled. That where there happens to be such a structure and conformation of the interior parts of the earth, as that the fire may pass freely, and without impediment, from the caverns wherein it assembles unto those spiracles, it then readily gets out from time to time, without shaking or disturbing the earth; but, where such communication is wanting, or passage not sufficiently large and open, so that it cannot come at the spiracles, it heaves up and shocks the earth with greater or lesser impetuosity, according to the quantity of fire thus assembled, till it has made its way to the mouth of the volcano. That therefore there are scarce any countries much annoyed by earthquakes, but have one of these fiery vents; which are constantly in flames when any earthquake happens; as disgorging that fire, which, whilst underneath, was the cause of the disaster. Lastly, that, were it not for these *diverticula*, it would rage in the bowels of the earth much more furiously, and make greater havoc than it doth.

We have seen what fire and water may do; and that either of them are sufficient for all the phenomena of earthquakes; if they should both fail, we have a third agent, scarce inferior to either of them; the reader must not be surprised when we tell him it is air.

Monsieur Amontons, in his *Mémoires de l'Académie des Sciences, An* 1703, has an express discourse to prove, that, on the foot of the new experiments of the weight and spring of the air, a moderate degree of heat may bring the air into a condition capable of causing earthquakes. It is shown, that, at the depth of 43,528 fathoms below the surface of the earth, air is only one fourth 'ess heavy than mercury. Now, this depth of 43,528 fathoms is only a seventy-fourth part of the

semidiameter of the earth. And the vast sphere beyond this depth, in diameter 6,451,538 fathoms, may probably be only filled with air; which will be here greatly condensed, and much heavier than the heaviest bodies we know in nature. But it is found by experiment, that, the more air is compressed, the more does the same degree of heat increase its spring, and the more capable does it render it of a violent effect; and that, for instance, the degree of heat of boiling water increases the spring of the air above what it has in its natural state, in our climate, by a quantity equal to a third of the weight wherewith it is pressed. Whence we may conclude, that a degree of heat, which on the surface of the earth will only have a moderate effect, may be capable of a very violent one below. And, as we are assured, that there are in nature degrees of heat much more considerable than that of boiling water, it is very possible there may be some whose violence, further assisted by the exceeding weight of the air, may be more than sufficient to break and overturn this solid orb of 43,528 fathoms; whose weight, compared to that of the included air, would be but a trifle.

Chemistry furnishes us a method of making artificial earthquakes, which shall have all the great effects of natural ones; which, as it may illustrate the process of nature in the production of these terrible phenomena under ground, we shall here add.

To twenty pounds of iron filings, add as many of sulphur; mix, work, and temper the whole together with a little water, so as to form a mass, half moist and half dry. This being buried three or four feet under ground, in six or seven hours time, will have a prodigious effect; the earth will begin to tremble, crack, and smoke, and fire and flame burst through.

Such is the effect even of the two cold bodies, in

cold ground; there only wants a sufficient quantity of this mixture to produce a true Ætna. If it were supposed to burst out under the sea, it would produce a spout; and, if it were in the clouds, the effect would be thunder and lightning.

An earthquake is defined to be a vehement shake, or agitation, of some considerable place, or part of the earth, from natural causes; attended with a huge noise like thunder, and frequently with an eruption of water, or fire, or smoke, or winds, &c.

They are the greatest and most formidable phenomena of nature. Aristotle and Pliny distinguish two kinds, with respect to the manner of the shake, viz. a tremor and a pulsation; the first being horizontal, in alternate vibrations, compared to the shaking of a person in an ague. The second perpendicular, up and down, their motion resembling that of boiling.

Agricola increases the number, and makes four kinds; which Albertus Magnus again reduces to three, viz. inclination, when the earth librates alternately from right to left, by which mountains have been sometimes brought to meet, and clash against each other; pulsation, when it beats up and down, like an artery; and trembling, when it shakes and totters every way, like a flame.

The Philosophical Transactions furnish us with abundance of histories of earthquakes; particularly one at Oxford, in 1665, by Dr. Wallis and Mr. Boyle. Another at the same place in 1683, by Mr. Pigot. Another in Sicily, in 1692–3, by Mr. Hartop, Father Alessandro Burgos and Vin. Bonajutus, which last is one of the most terrible ones in all history.

It shook the whole island; and not only that, but Naples and Malta shared in the shock. It was of the second kind mentioned by Aristotle and Pliny, viz. a

perpendicular pulsation or succussion. It was impossible, says the noble Bonajutus, for anybody in this country to keep on their legs, on the dancing earth; nay, those that lay on the ground, were tossed from side to side, as on a rolling billow; high walls leaped from their foundations several paces.

The mischief it did is amazing; almost all the buildings in the countries were thrown down. Fifty-four cities and towns, besides an incredible number of villages, were either destroyed or greatly damaged. We shall only instance the fate of Catania, one of the most famous, ancient, and flourishing cities in the kingdom; the residence of several monarchs, and a university. "This once famous, now unhappy Catania," to use words of Father Burgos, "had the greatest share in the tragedy. Father Antonio Serovita, being on his way thither, and at the distance of a few miles, observed a black cloud, like night, hovering over the city; and there arose from the mouth of Mongibello great spires of flame, which spread all around. The sea all of a sudden began to roar, and rise in billows; and there was a blow, as if all the artillery in the world had been at once discharged. The birds flew about astonished, the cattle in the fields ran crying, &c. His and his companion's horse stopped short, trembling; so that they were forced to alight. They were no sooner off, but they were lifted from the ground above two palms. When casting his eyes towards Catania, he with amazement saw nothing but a thick cloud of dust in the air. This was the scene of their calamity; for of the magnificent Catania, there is not the least footstep to be seen." Bonajutus assures us, that, of 18,914 inhabitants, 18,000 perished therein. The same author, from a computation of the inhabitants, before and after

the earthquake, in the several cities and towns, finds that near 60,000 perished out of 254,900.

Jamaica is remarkable for earthquakes. The inhabitants, Dr. Sloane informs us, expect one every year. That author gives us the history of one in 1687; another horrible one, in 1692, is described by several anonymous authors. In two minutes time it shook down and drowned nine tenths of the town of Port Royal. The houses sunk outright, thirty or forty fathoms deep. The earth opening swallowed up people, and they rose in other streets; some in the middle of the harbour, and yet were saved; though there were two thousand people lost, and one thousand acres of land sunk. All the houses were thrown down throughout the island. One Hopkins had his plantation removed half a mile from its place. Of all wells, from one fathom to six or seven, the water flew out at the top with a vehement motion. While the houses on the one side of the street were swallowed up, on the other they were thrown on heaps; and the sand in the street rose like waves in the sea, lifting up everybody that stood on it, and immediately dropping down into pits; and at the same instant, a flood of waters breaking in rolled them over and over; some catching hold of beams and rafters, &c. Ships and sloops in the harbour were overset and lost; the Swan frigate particularly, by the motion of the sea, and sinking of the wharf, was driven over the tops of many houses.

It was attended with a hollow rumbling noise like that of thunder. In less than a minute three quarters of the houses, and the ground they stood on, with the inhabitants, were all sunk quite under water; and the little part left behind was no better than a heap of rubbish. The shake was so violent, that it threw people down on their knees, or their faces, as they were

running about for shelter. The ground heaved and swelled like a rolling sea, and several houses, still standing, were shuffled and moved some yards out of their places. A whole street is said to be twice as broad now as before; and in many places the earth would crack, and open, and shut, quick and fast. Of which openings, two or three hundred might be seen at a time; in some whereof, the people were swallowed up; others the closing earth caught by the middle, and pressed to death; in others, the heads only appeared. The larger openings swallowed up houses; and out of some would issue whole rivers of waters, spouted up a great height into the air, and threatening a deluge to that part the earthquake spared. The whole was attended with stenches and offensive smells, the noise of falling mountains at a distance, &c., and the sky in a minute's time was turned dull and reddish, like a glowing oven. Yet, as great a sufferer as Port Royal was, more houses were left standing therein than on the whole island beside. Scarce a planting-house or sugar-work was left standing in all Jamaica. A great part of them were swallowed up, houses, people, trees, and all at one gape; in lieu of which, afterwards appeared great pools of water, which, when dried up, left nothing but sand, without any mark that ever tree or plant had been thereon.

Above twelve miles from the sea, the earth gaped and spouted out, with a prodigious force, vast quantities of water into the air; yet the greatest violences were among the mountains and rocks; and it is a general opinion, that, the nearer the mountains, the greater the shake; and that the cause thereof lay there. Most of the rivers were stopped up for twenty-four hours by the falling of the mountains, till, swelling up, they found themselves new tracts and channels, tearing up in their

passage trees, &c. After the great shake, those people who escaped got on board ships in the harbour, where many continued above two months; the shakes all that time being so violent, and coming so thick, sometimes two or three in an hour, accompanied with frightful noises, like a ruffling wind, or a hollow, rumbling thunder, with brimstone blasts, that they durst not come ashore. The consequence of the earthquake was a general sickness, from the noisome vapors belched forth, which swept away above three thousand persons.

After the detail of these horrible convulsions, the reader will have but little curiosity left, for the less considerable phenomena of the earthquake at Lima, in 1687, described by Father Alvarez de Toledo, wherein above five thousand persons were destroyed; this being of the vibratory kind, so that the bells in the church rung of themselves; or that at Batavia in 1699, by Witsen; that in the north of England, in 1703, by Mr. Thoresby; or, lastly, those in New England, in 1663 and 1670, by Dr. Mather.

A PROPOSAL

FOR PROMOTING USEFUL KNOWLEDGE AMONG THE BRITISH PLANTATIONS IN AMERICA.*

Philadelphia, 14 May, 1743.

THE English are possessed of a long tract of continent, from Nova Scotia to Georgia, extending north and south through different climates, having different soils, producing different plants, mines, and minerals, and capable of different improvements, manufactures, &c.

The first drudgery of settling new colonies, which confines the attention of people to mere necessaries, is now pretty well over; and there are many in every province in circumstances, that set them at ease, and afford leisure to cultivate the finer arts, and improve the common stock of knowledge. To such of these who are men of speculation, many hints must from time to time arise, many observations occur, which if well examined, pursued, and improved, might produce discoveries to the advantage of some or all of the British plantations, or to the benefit of mankind in general.

But as, from the extent of the country, such persons

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* This paper appears to contain the first suggestion, in any public form, for an *American Philosophical Society*. It was originally printed on a separate sheet, as a circular letter, and sent by the author to his different correspondents. The copy, from which the above is printed, has been found among the papers of Cadwallader Colden, in New York. On the same sheet is a manuscript letter from John Bartram, the celebrated botanist, to Mr. Colden, dated at Philadelphia, March 27th, 1744, nearly a year after the above "*Proposal*" was issued. Mr. Bartram says in that letter; "I here send one of our *Proposals* for forming a Philosophical Society. We have already had three meetings, and several learned and curious persons from our neighbouring colonies have already joined membership with us, and we hope you will please to do us the honor to be enrolled in our number. I hope this undertaking may be of public benefit to our American colonies, if we act with diligent application."—EDITOR.

are widely separated, and seldom can see and converse, or be acquainted, with each other, so that many useful particulars remain uncommunicated, die with the discoverers, and are lost to mankind; it is, to remedy this inconvenience for the future, proposed,

That one society be formed of *virtuosi* or ingenious men, residing in the several colonies, to be called *The American Philosophical Society*, who are to maintain a constant correspondence.

That Philadelphia, being the city nearest the centre of the continent colonies, communicating with all of them northward and southward by post, and with all the islands by sea, and having the advantage of a good growing library, be the centre of the Society.

That at Philadelphia there be always at least seven members, viz. a physician, a botanist, a mathematician, a chemist, a mechanician, a geographer, and a general natural philosopher, besides a president, treasurer, and secretary.

That these members meet once a month, or oftener, at their own expense, to communicate to each other their observations and experiments, to receive, read, and consider such letters, communications, or queries as shall be sent from distant members; to direct the dispersing of copies of such communications as are valuable, to other distant members, in order to procure their sentiments thereupon.

That the subjects of the correspondence be, all new-discovered plants, herbs, trees, roots, their virtues, uses, &c.; methods of propagating them, and making such as are useful, but particular to some plantations, more general; improvements of vegetable juices, as ciders, wines, &c.; new methods of curing or preventing diseases; all new-discovered fossils in different countries, as mines, minerals, and quarries; new and useful

improvements in any branch of mathematics; new discoveries in chemistry, such as improvements in distillation, brewing, and assaying of ores; new mechanical inventions for saving labor, as mills and carriages, and for raising and conveying of water, draining of meadows, &c.; all new arts, trades, and manufactures, that may be proposed or thought of; surveys, maps, and charts of particular parts of the sea-coasts or inland countries; course and junction of rivers and great roads, situation of lakes and mountains, nature of the soil and productions; new methods of improving the breed of useful animals; introducing other sorts from foreign countries; new improvements in planting, gardening, and clearing land; and all philosophical experiments that let light into the nature of things, tend to increase the power of man over matter, and multiply the conveniences or pleasures of life.

That a correspondence, already begun by some intended members, shall be kept up by this Society with the ROYAL SOCIETY of London, and with the DUBLIN SOCIETY.

That every member shall have abstracts sent him quarterly, of every thing valuable communicated to the Society's Secretary at Philadelphia; free of all charge except the yearly payment hereafter mentioned.

That, by permission of the postmaster-general, such communications pass between the Secretary of the Society and the members, postage-free.

That, for defraying the expense of such experiments as the Society shall judge proper to cause to be made, and other contingent charges for the common good, every member send a piece of eight per annum to the treasurer, at Philadelphia, to form a common stock, to be disbursed by order of the President with the consent of the majority of the members that can conveniently

be consulted thereupon, to such persons and places where and by whom the experiments are to be made, and otherwise as there shall be occasion; of which disbursements an exact account shall be kept, and communicated yearly to every member.

That, at the first meetings of the members at Philadelphia, such rules be formed for regulating their meetings and transactions for the general benefit, as shall be convenient and necessary; to be afterwards changed and improved as there shall be occasion, wherein due regard is to be had to the advice of distant members.

That, at the end of every year, collections be made and printed, of such experiments, discoveries, and improvements, as may be thought of public advantage; and that every member have a copy sent him.

That the business and duty of the Secretary be, to receive all letters intended for the Society, and lay them before the President and members at their meetings; to abstract, correct, and methodize such papers as require it, and as he shall be directed to do by the President, after they have been considered, debated, and digested in the Society; to enter copies thereof in the Society's books, and make out copies for distant members; to answer their letters by direction of the President, and keep records of all material transactions of the Society.

Benjamin Franklin, the writer of this Proposal, offers himself to serve the Society as their secretary, till they shall be provided with one more capable.*

* This is doubtless the paper alluded to by Dr. Franklin in his autobiography, where he says; "I succeeded, in the year 1744, in proposing and establishing a *Philosophical Society*. The paper I wrote for that purpose will be found among my writings; if not lost with many others." —EDITOR.

FROM CADWALLADER COLDEN TO B. FRANKLIN.*

A New Method of Printing.

October, 1743.

SIR,

As the art of printing has, without question, been of very great use in advancing learning and knowledge, the abuse of it, as of all other good things, has likewise produced many inconveniences. The number of books printed on the same subject, most of which are nothing but unskilful and erroneous copies of good works, written only for ostentation of learning, or for sordid profit,

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* This letter was first made public in the *American Medical and Philosophical Register*, a periodical work edited by Dr. David Hosack and Dr. John W. Francis. The first number of this journal appeared in July, 1810. It was continued for several years, and, although chiefly devoted to medical science and practice, it contains many valuable papers on other subjects, illustrative of the progress of knowledge in the United States, and proving the ability, learning, and research of the editors. It possesses a special interest for the general reader in the biographical sketches of eminent men, drawn from accurate and original materials, which are inserted in different parts. Among these is a spirited and well written memoir of Cadwallader Colden, from the pen of Dr. Francis, the more important as the author had access to a large collection of manuscript papers in possession of the Colden family.

Cadwallader Colden was born in Scotland, on the 17th of February, 1688. He was educated at the University of Edinburgh. He came over to Philadelphia in the year 1708, where he practised physic till 1715, when he made a voyage to Europe, but returned in 1718, and settled in the city of New York. He soon after relinquished the practice of physic, and entered upon a public career, holding successively the offices of surveyor-general of the province, master in chancery, member of the council, and lieutenant-governor. This last post he occupied for fifteen years, during a large part of which period the administration of the government devolved upon him, on account of the death or absence of the governors. "His political character was rendered very conspicuous by the firmness of his conduct during the violent commotions which preceded the Revolution. His administration is also rendered memorable among other things for several charters of incorporation for useful and benevolent purposes." He resided many years of his life at his place,

renders the path to knowledge very intricate and tedious. The reader who has no guide, and the greatest number have none, is lost in the wilderness of numberless books. He is most commonly led astray by the glaring appearances of title-pages and other artifices of the mystery of bookselling.

It is likewise a common complaint, that a poor author makes nothing near the profit that the bookseller does of his labor; and probably the more pains the author has taken, the more difficult the performance, and the more masterly it is done, the less profit to him; for the good books, like jewels, never lose their intrinsic value; yet they have fewer purchasers than Bristol stones, and the sale of them is slow.

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called Coldenham, a few miles from Hudson's River, west of Newburg. He died at his seat on Long Island, September 28th, 1776, in the eighty-ninth year of his age.

"Though his principal attention," says Dr. Francis, "after the year 1760, was necessarily directed from philosophical to political matters, yet he maintained with great punctuality his literary correspondence, particularly with Linnæus of Upsal, Gronovius of Leyden, Dr. Porterfield and Dr. Whytt of Edinburgh, Dr. Fothergill and Mr. Collinson of London. There were also several communications on mathematical and astronomical subjects between him and the Earl of Macclesfield. With most of the eminent men of our own country he held an almost uninterrupted epistolary correspondence. Among them we may mention the names of Dr. Garden, Mr. J. Bartram, Dr. Douglass, Dr. John Bard, Dr. Samuel Bard, James Alexander, and Dr. Franklin. With Dr. Franklin in particular he was a constant and intimate correspondent, and they regularly communicated to each other their philosophical and physical discoveries, especially on electricity. In their letters are to be observed the first dawnings of many of those discoveries, which Dr. Franklin has communicated to the world, and which so much astonished and benefited mankind."

He wrote many treatises on medical, mathematical, and philosophical subjects, and also a well known "History of the Five Indian Nations." He left several manuscript papers, which are curious and valuable. He was particularly skilled in botany and the mathematical sciences. In short, for the great variety and extent of his learning, his unwearied research, his talents, and the public sphere which he filled, Cadwallader Colden may justly be placed in a high rank among the distinguished men of his time. — EDITOR.

As the lessening or removing of some of these inconveniences may be of use to the republic of letters, I hope to be excused in making the following attempt for that purpose, by proposing *a new method of printing.*

Let there be made of some hard metal, such as copper or brass, a number of types, or rather matrices, on the face of each of which, one letter of the alphabet is to be imprinted *en creux*, by a stamp, or such other method by which the matrices for founding of types are commonly made. They must be all of the same dimension, as to breadth and thickness, with that of types, but half their length seems sufficient. Their sides must be so equal and smooth as to leave no vacuity between them when joined. There must likewise be a sufficient number of each letter or character to compose at least one page in octavo of any book.

These matrices, I suppose, may be cast in a mould, or a plate of copper may be divided exactly into squares, and the letter or character be stamped into the middle of each square, and the squares afterwards cut asunder by a proper saw. The best method of making these will be easily discovered by those whose business it is to make founts for printing-types.

When a sufficient number of each letter and character is obtained, they are to be placed in the same manner the types are, when composed for printing, only that they must all stand directly as they are read, and as they will appear afterwards on the paper.

The composure of one page, after it is carefully corrected, is to be placed in a case or mould, fitted to it, of the length and breadth of the page, and of such depth as to cast a plate a quarter of an inch thick, which will perfectly represent a page composed in the common manner for printing.

As to the art of casting the plate perfect, founders

and type-makers must be consulted for the composition of the metal, and for the flux for running it clean and clear, so that no vacuities be left; for which purpose, I am told that the funnel, by which the melted metal is poured in, being made large, and the filling it with the melted metal after the mould is full, are of use to make the letter everywhere full and complete. For, by the weight of the metal in the funnel, the liquid metal in the mould is pressed into every crevice. The funnel's extending the whole length of one of the sides gives, likewise, free vent to the air.

Or, after a page shall be composed, as before mentioned, and the types and matrices well secured in a frame upon a strong plate, they may, by a screw, be pressed upon a sheet of melted lead, and thereby a plate of lead be procured, representing, as the former, a page composed of types for printing. Which of the methods are most practicable, artists can best determine.

After the page shall be thus formed, the matrices may be loosened and dispersed into their proper boxes, and may serve for as many other pages as types in common printing do.

When a number of pages, sufficient for a sheet, are thus made, they may be carried to any printing-press, and such a number of sheets as shall be thought proper be cast off, and then be laid by till more copies be wanted.

I choose an octavo page, because, if the page-title and page-number be left out, as likewise the directions and signatures at the foot of the page, by joining two pages together, it may be made a quarto, or by joining four, a folio. Thus several editions, in octavo, quarto, and folio, may at once be made, to suit every buyer's humor.

The page-titles, number, and bottom signatures may

be cast in small moulds apart, and joined, as may be proper.

The most convenient size of a page is that of small paper, so as to fill it up, and to leave very little margin; then by adding the page-titles, or marginal notes, or notes at the bottom, all cast in frames separately, the large paper may be sufficiently filled.

I believe that this method of printing, every thing considered, will not be more chargeable than the common method. A thousand, or some thousands sometimes, of copies are cast off at once in the common method, and the paper and pressman's labor of what is not speedily sold may or must lie dead for some years; whereas, in this method, no more need be cast off at a time than may well be supposed to sell speedily. If I be not mistaken, the metal necessary for one sheet will not exceed the value of four hundred sheets of paper, and in the common method several hundred sheets lie useless for, sometimes, many years. If the book should not answer, there is a great loss in the paper; whereas the metal used in this method retains its intrinsic value.

I shall instance some of the advantages in this method, which induce me to communicate my thoughts to others.

1. An author by this means can secure the property of his own labor.

2. A correct edition is at all times secured, and therefore may be useful in the classics, trigonometrical tables, &c.

3. A weak and ignorant attempt on the same subject will be discouraged; for, as a new edition of a valuable book is continually secured, without any new expense, booksellers will not readily hazard the publishing of books of the same nature.

4. But what I chiefly value this method of printing for, is, from the advantages it gives an author in making his work perfect, and in freeing it from mistakes; for, by printing off a few copies of any sheet, and sending them among his friends, and by suffering them to fall into the hands of a malevolent critic, he may have an opportunity of correcting his mistakes, before they appear to the world. By the same means he may make his work more complete than otherwise he could, by the assistance which his friends may give him in several parts of it. It is for these reasons chiefly, that I propose the plates not to exceed an octavo page, and to have no signatures; for in case of a mistake, the loss of one page may correct the error, and, where improvements or additions are necessary, as many pages may be intermixed as shall be necessary, without any inconvenience, and small explications may be made by the marginal notes.

Lastly. The greatest advantage I conceive will be in the learned sciences; for they often require a long time to bring them to perfection, and require the assistance of others in many particulars. Many a valuable piece has been lost to the world by the author dying before he could bring his work to the perfection he designed. Now, by the assistance which he may have by this method from others, this time may be much shortened, and the progress he has made may be preserved for others to continue in case of his death. An author may publish his work in parts, and shall continue, in many cases, to complete and make them more perfect without any loss of what was done before. By this method likewise, a man of learning, when poor, may leave some part of his estate in his own way for a child, as mechanics often do for theirs.

Whether the method I propose will answer the end

designed, or whether it be practicable, I cannot with sufficient assurance say; because we have no artists in this country who can make the experiment, neither can they have encouragement sufficient to tempt them to make the trial. However, I hope to be excused, by the use of the design, and as it may chance to give some hint to a skilful person to perform effectually what I only aim at in vain.

If the charge of lead or metal plates be thought too great, I know not but that the impression may be made on thin planes of some kinds of wood, such as lime tree or poplar, which have a soft, smooth grain when green, and are hard and smooth when dry.

Ever since I had the pleasure of a conversation with you, though very short, by our accidental meeting on the road, I have been very desirous to engage you in a correspondence. You were pleased to take some notice of a method of printing, which I mentioned to you at that time, and to think it practicable. I have no further concern for it than as it may be useful to the public; my reasons for thinking so, you will find in the enclosed copy of a paper, which I last year sent to Mr. Collinson in London. Perhaps my fondness for my own conceptions may make me think more of it than it deserves, and may make me jealous that the common printers are willing to discourage, out of private interest, any discovery of this sort. But as you have given me reason to think you zealous in promoting every useful attempt, you will be able absolutely to determine my opinion of it.

I long very much to hear what you have done in your scheme of erecting a society at Philadelphia for promoting of useful arts and sciences in America. If you think of any thing in my power, whereby I can promote so useful an undertaking, I will with much

pleasure receive your instructions for that end. As my son Cadwallader bears this, I thereby think myself secured of the pleasure of a line from you by him.*

TO CADWALLADER COLDEN.

Philadelphia, 4 November, 1743.

SIR,

I received the favor of yours, with the proposal for a new method of printing, which I am much pleased with; and, since you express some confidence in my opinion, I shall consider it very attentively and particularly, and in a post or two send you some observations on every article.†

My long absence from home in the summer put my business so much behindhand, that I have been in a continual hurry ever since my return, and had no leisure to forward the scheme of the Society. But that hurry being now near over, I purpose to proceed in the affair very soon, your approbation being no small encouragement to me.

I cannot but be fond of engaging in a correspondence so advantageous to me as yours must be. I shall always receive your favors as such, and with great pleasure.

I wish I could by any means have made your son's longer stay here as agreeable to him, as it would have been to those who began to be acquainted with him.

I am, Sir, with much respect,

Your most humble servant,

B. FRANKLIN.

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* The process here described, which was evidently an original invention of Mr. Colden, has some resemblance to the early attempts at stereotype printing. — EDITOR.

† These observations have not been found. — EDITOR.

FROM CADWALLADER COLDEN TO W. STRAHAN.

Remarks on a New Method of Printing, in Reply to Mr. Strahan's Objections.

3 December, 1743.

SIR,

As I think myself much obliged to you for the trouble you have taken to answer a paper I formerly wrote to Mr. Collinson, I take the first opportunity to acknowledge it. I am fully convinced, that your reasonings are just; and, as they have been likewise confirmed by experience, they leave no room to doubt.* But, at the same time, I must inform you, that you have not entirely taken my view. I easily, and at first, perceived that this method would not succeed for common books, which generally bear but one edition, and are chiefly calculated for the present times, and with a view to a speedy profit, while the present taste and humor lasts. I had confined my view to particular cases, and to a narrow compass, to books in the sciences, and to such only, which have an intrinsic value independent of the governing humor or taste, the value of which is known to few, but which will always be esteemed and sought after by some.

Of such sort is Euclid's Elements, which has continued in the esteem of the world above two thousand years. Such likewise are Sir Isaac Newton's "Optics," and his "Principia," which will for all ages be called for by the few that understand them. Such likewise are Trigonometrical Tables, &c., which never can be out of fashion, or out of use, and which may be affixed to,

* The substance of Mr. Strahan's objections to the new method of printing had been communicated to Mr. Colden by Mr. Collinson.—EDITOR.

and are convenient for, many different books. And yet all these books, and others of the kind, have but a slow sale. The composing of them for the press must likewise be more difficult and expensive, because the composer cannot be assisted by the sense, and a mistake in one letter or figure frequently disturbs the whole. Some of these have been often cut on copper plates (with profit I suppose to the owners), and yet certainly they might be impressed on printing metal by types *en creux* in the method I propose, with much less expense than by engraving and by the rolling press.

I accidentally, last summer, fell into company with a printer, the most ingenious in his way, without question, of any in America. Upon my mentioning my thoughts, which I wrote to Mr. Collinson, he told me of the method that had been used in Holland, which you likewise mention, but he thought the method by types *en creux* to be an improvement of that method. And, as he is a man very lucky in improving every hint, he has done something on this foundation, which I have seen, and which has puzzled all the printers in this country to conceive by what method it is done. As printing is this man's trade, and he makes a benefit of it, I do not think myself at liberty to communicate it without his consent, though, as to my own part, I have no interest in keeping the secret; nor had I, nor have I, any other view in what I formerly wrote, than to communicate a thought which I fancied might be of use to others.

What I have wrote perhaps may occasion some reflections to you, that may not prove of loss to you in your way. If I can make any return for your civility by any service in this country, you will oblige me by giving me an opportunity to do it, and shall be glad of

any occasion worth your while to continue the correspondence. If you think proper to inform me of the common charge of printing, and the price of common printing-paper, perhaps it may give me an opportunity, some time or other, of serving you.

I am, &c.

CADWALLADER COLDEN.

TO CADWALLADER COLDEN.*

Organization of a Philosophical Society.

New York, 5 April, 1744.

SIR,

Happening to be in this city about some particular affairs, I have the pleasure of receiving yours of the 28th past, here; and can now acquaint you, that the Society, as far as it relates to Philadelphia, is actually formed, and has had several meetings to mutual satisfaction. As soon as I get home, I shall send you a short account of what has been done and proposed at those meetings. The members are;

Dr. Thomas Bond, as Physician.

Mr. John Bartram, as Botanist.

Mr. Thomas Godfrey, as Mathematician.

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* This letter was first printed in the *American Medical and Philosophical Register* for October, 1811. The manuscript was obtained by the editors from the papers of Cadwallader Colden. Accompanying the printed letter is a beautiful *fac-simile* of the original in the handwriting of Franklin. The AMERICAN PHILOSOPHICAL SOCIETY, as afterwards instituted, was formed out of two Societies, of which the above was one. The other was the *Society for promoting and propagating Useful Knowledge*. The two Societies were incorporated into one, called the AMERICAN PHILOSOPHICAL SOCIETY, in December, 1768; and in January, 1769, Franklin was elected the first President, although he was at that time in England.—EDITOR.

Mr. Samuel Rhoads, as Mechanician.
Mr. William Parsons, as Geographer.
Dr. Phineas Bond, as General Natural Philosopher.
Mr. Thomas Hopkinson, President.
Mr. William Coleman, Treasurer.
B. F——, Secretary.

To whom the following members have since been added, viz. Mr. Alexander, of New York; Mr. Morris, Chief Justice of the Jerseys; Mr. Home, Secretary of do.; Mr. John Coxe, of Trenton; and Mr. Martyn, of the same place. Mr. Nicholls tells me of several other gentlemen of this city, that incline to encourage the thing; and there are a number of others, in Virginia, Maryland, and the New England colonies, whom we expect to join us, as soon as they are acquainted that the Society has begun to form itself.

I am, Sir, with much respect,

Your most obedient humble servant,

B. FRANKLIN.

FROM WILLIAM STRAHAN TO C. COLDEN.

Objections to the proposed New Method of Printing.

London, 9 May, 1744.

SIR,

I was favored with yours of the 3d of December last, in answer to a line I sent you by Mr. Collinson's direction. The desire you have of promoting any useful discovery is very laudable; but in the present case I own I do not think it can be of the benefit you seem to imagine. For as to the books you mention, though they are of great and lasting utility, their sale is very insignificant; besides that they are the properties of

particular persons, who have always a number of them sufficient to supply the market, and therefore would not easily, if at all, be induced to try any other method of printing them than what they have been used to.

In the mean time, I make no doubt but what you propose might be useful in some sorts of Tables, such as Tables of Interest, Logarithms, &c., provided the first expense was not too great. At present the printers here keep several things, which are often wanted, continually standing; such as "The Psalms" in metre, in all sizes, "The Proverbs of Solomon," several school books, and classic authors, "The Child's Guide," the "Catechisms," &c. &c., till the types are quite worn out, which, you know, answers the very same end as the method you propose. As to what you mention of some sorts of wood, I own I do not know any thing of the way you would use them, and therefore can form no judgment of their usefulness, nor do I desire or expect you should discover any thing of it to me, without the consent of the person you mention, who has already tried it, and whose business it is to profit by any discovery his uncommon abilities may enable him to make. From the character you give him, I am sure it must be Mr. Franklin you mean, whose fame has long ago reached this part of the world, for a most ingenious man "in his way." I have had the pleasure of corresponding with him lately, and have sent him by *The Mercury*, Captain Hargrave, one of my journeymen, to whom he intends to give the management of one of his printing-houses. His name is David Hall.* If he is

* This person was long associated in business with Franklin. In his autobiography he characterizes David Hall as "a very able, honest, and industrious partner"; and adds, "he took off my hands all care of the printing-office, paying me punctually my share of the profits. This partnership continued eighteen years, successfully for us both."—Editor.

settled near you, as is probable, or if you should chance to meet with him anywhere else, I should be extremely obliged to you if you would show him any civility in your power, as he is a stranger, and a most deserving young man. He can inform you fully how printing affairs stand here.

I am greatly indebted to you for your kind offer of serving me. The prices of paper and printing here are now very reasonable. Good printing demy paper (which is the size commonly used) may be had at ten, eleven, or twelve shillings a ream, and other sizes in proportion, viz. crown paper at seven or eight shillings, and pot paper at five or six shillings a ream. Printing one thousand copies of a sheet on a pica letter costs a guinea, and one shilling for every one hundred over that number; and so in proportion, according to the size of the type and page. A pica octavo page contains thirty-eight lines.

I likewise sell all sorts of books, so that if any of your acquaintance want any, I shall be obliged to you if you will direct them to me, in Wine Office Court in Fleet Street. I shall be very well pleased if, in return for your kindness, I could be of any service to you in this place. Meanwhile I shall be extremely glad to keep a friendly correspondence with you, and am, Sir,

Your most obedient servant,

WILLIAM STRAHAN.

FROM CADWALLADER COLDEN TO B. FRANKLIN.

Concerning Thoughts on different Species of Matter.— Suggestion for publishing Philosophical Papers.

Coldenham, December, 1744.

SIR,

The season of the year advancing in which our correspondence from this place with New York becomes more uncertain, and my eldest son going now to New York, where he proposes to stay eight or ten days, I hope you will excuse my interrupting you in your business, which I know allows you little time for trifles or amusements.

In your last you gave me hopes, that you would soon be able to inform me what sentiment Mr. Logan entertains of the Introduction to Fluxions, which was submitted to his perusal. By my last I transmitted to you some thoughts of the different species of matter. As these thoughts are entirely new, and out of the common road of thinking, I have reason not only to be apprehensive that others may not easily receive the conceptions, but that I may have imposed on myself; and it is for this reason, that I have submitted them to Mr. Logan's and your examination. I have already shown it to Mr. Alexander, and some steps I have made in applying these thoughts to the explanation of some pnenomena in which philosophers have hitherto not been able to give satisfaction. He has taken much more pains in the examination, than could have been expected in one so deeply engaged in business; and, however pleasing his sentiments may be to me, I have reason to suspect that he may be biassed by favor to a very long and intimate acquaintance.

I long likewise to know what progress you make in

forming your Society. If it meets with obstruction from the want of proper encouragement or otherwise, I would have you attempt some other method of proceeding in your design; for I shall be very sorry to have it entirely dropped. May you not, as printer, propose to print at certain times a collection of such pieces on the subject of your former proposals, which any shall think proper to send you, and, by way of specimen, to print such papers as your friends may have communicated to you in consequence of your proposals? For this purpose you may desire a subscription by all persons indifferently for your encouragement. I do not propose, that every thing be printed that shall be sent. You may communicate them to the best judges with you of the several subjects on which these papers shall happen to be written, where you are not willing entirely to trust to your own judgment; and, if they be found not fit for the press, you may return them with remarks, or make some excuse for not publishing them. This I expect will in time produce a Society as proposed, by giving men of learning or genius some knowledge of one another, and will avoid some difficulties that always attend the forming of societies in their beginning. Three hundred copies may be sufficient at first, till it be discovered what encouragement the undertaking meets with; and such a number, I cannot doubt, will sell.

I am, &c.

CADWALLADER COLDEN.

AN

ACCOUNT

OF THE NEW-INVENTED

PENNSYLVANIAN FIRE-PLACES;

WHEREIN

THEIR CONSTRUCTION AND MANNER OF OPERATION IS PARTICULARLY EXPLAINED; THEIR ADVANTAGES ABOVE EVERY OTHER METHOD OF WARMING ROOMS DEMONSTRATED; AND ALL OBJECTIONS THAT HAVE BEEN RAISED AGAINST THE USE OF THEM ANSWERED AND OBVIATED. WITH DIRECTIONS FOR PUTTING THEM UP, AND FOR USING THEM TO THE BEST ADVANTAGE. AND A COPPER-PLATE, IN WHICH THE SEVERAL PARTS OF THE MACHINE ARE EXACTLY LAID DOWN, FROM A SCALE OF EQUAL PARTS.

PHILADELPHIA;

PRINTED AND SOLD BY B. FRANKLIN, 1744.

Several editions of this tract have been published, in Europe and America, and sometimes with a wrong date. The above title is a transcript from that of the first edition. In his autobiography he thus speaks of this performance. "Having, in 1742, invented an open stove for the better warming of rooms, and at the same time saving fuel, as the fresh air admitted was warmed in entering, I made a present of the model to Mr. Robert Grace, one of my early friends, who, having an iron furnace, found the casting of the plates for these stoves a profitable thing, as they were growing in demand. To promote that demand, I wrote and published a pamphlet, intitled '*An Account of the new-invented Pennsylvanian Fireplaces*,' &c. This pamphlet had a good effect. Governor Thomas was so pleased with the construction of this stove, as described in it, that he offered to give me a patent for the sole vending of them for a term of years; but I declined it from a principle, which has ever weighed with me on such occasions, viz. *That, as we enjoy*

great advantages from the inventions of others, we should be glad of an opportunity to serve others by any invention of ours; and this we should do freely and generously.

"An ironmonger in London, however, assuming a good deal of my pamphlet, and working it up into his own, and making some small changes in the machine, which rather hurt its operation, got a patent for it there, and made, as I was told, a little fortune by it. And this is not the only instance of patents taken out of my inventions by others, though not always with the same success; which I never contested, as having no desire of profiting by patents myself, and hating disputes. The use of these fire-places in very many houses, both here in Pennsylvania, and the neighbouring states, has been, and is, a great saving of wood to the inhabitants."

In an edition of the author's writings on electrical and philosophical subjects, published in London in the year 1769, the following note is appended to this tract.

"Soon after the foregoing piece was published, some persons in England, in imitation of Mr. Franklin's invention, made what they call '*Pennsylvanian Fire-places, with Improvements*'; the principal of which pretended improvements is, a contraction of the passages in the air-box, originally designed for admitting a quantity of fresh air, and warming it as it entered the room. The contracting these passages gains indeed more room for the grate, but in a great measure defeats their intention. For, if the passages in the air-box do not greatly exceed in dimensions the amount of all the crevices by which cold air can enter the room, they will not considerably prevent, as they were intended to do, the entry of cold air through these crevices."—EDITOR.

IN these northern colonies the inhabitants keep fires to sit by generally seven months in the year; that is, from the beginning of October to the end of April; and, in some winters, near eight months, by taking in part of September and May.

Wood, our common fuel, which within these hundred years might be had at every man's door, must now be fetched near one hundred miles to some towns, and makes a very considerable article in the expense of families.

As therefore so much of the comfort and conveniency of our lives, for so great a part of the year, depends on the article of *fire;* since fuel is become so expensive, and, as the country is more cleared and settled, will of course grow scarcer and dearer, any new proposal for saving the wood, and for lessening the charge and augmenting the benefit of fire, by some particular method of making and managing it, may at least be thought worth consideration.

The new fire-places are a late invention to that purpose, of which this paper is intended to give a particular account.

That the reader may the better judge, whether this method of managing fire has any advantage over those heretofore in use, it may be proper to consider both the old and new methods, separately and particularly, and afterwards make the comparison.

In order to this it is necessary to understand well, some few of the properties of air and fire, viz.

1. Air is rarefied by *heat*, and condensed by *cold*, that is, the same quantity of air takes up more space when warm than when cold. This may be shown by several very easy experiments. Take any clear glass bottle (a Florence flask stript of the straw is best), place it before the fire, and, as the air within is warmed and rarefied, part of it will be driven out of the bottle; turn it up, place its mouth in a vessel of water, and remove it from the fire; then, as the air within cools and contracts, you will see the water rise in the neck of the bottle, supplying the place of just so much air as was driven out. Hold a large hot coal near the side of the bottle, and, as the air within feels the heat, it will again distend and force out the water. Or, fill a bladder not quite full of air, tie the neck tight, and lay it before a fire as near as may be without scorching the

bladder; as the air within heats, you will perceive it to swell and fill the bladder, till it becomes tight, as if full blown; remove it to a cool place, and you will see it fall gradually, till it becomes as lank as at first.

2. Air rarefied and distended by heat is specifically* lighter than it was before, and will rise in other air of greater density. As wood, oil, or any other matter specifically lighter than water, if placed at the bottom of a vessel of water will rise till it comes to the top; so rarefied air will rise in common air, till it either comes to air of equal weight, or is by cold reduced to its former density.

A fire, then, being made in any chimney, the air over the fire is rarefied by the heat, becomes lighter, and therefore immediately rises in the funnel, and goes out; the other air in the room (flowing towards the chimney) supplies its place, is rarefied in its turn, and rises likewise; the place of the air thus carried out of the room, is supplied by fresh air coming in through doors and windows, or, if they be shut, through every crevice with violence, as may be seen by holding a candle to a key-hole. If the room be so tight as that all the crevices together will not supply so much air as is continually carried off, then, in a little time, the current up the funnel must flag, and the smoke, being no longer driven up, must come into the room.

1. Fire (that is, common fire) throws out light, heat, and smoke (or fume.) The two first move in right lines, and with great swiftness; the latter is but just separated from the fuel, and then moves only as it is carried by the stream of rarefied air; and without a continual accession and recession of air, to carry off

* Body or matter of any sort is said to be *specifically* heavier or lighter than other matter, when it has more or less substance or weight in the same dimensions.

the smoky fumes, they would remain crowded about the fire, and stifle it.

2. Heat may be separated from the smoke as well as from the light, by means of a plate of iron, which will suffer heat to pass through it without the others.

3. Fire sends out its rays of heat, as well as rays of light, equally every way; but the greatest sensible heat is over the fire, where there is, besides the rays of heat shot upwards, a continual rising stream of hot air, heated by the rays shot round on every side.

These things being understood, we proceed to consider the fire-places heretofore in use, viz.

1. The large open fire-places used in the days of our fathers, and still generally in the country, and in kitchens.

2. The newer-fashioned fire-places, with low breasts and narrow hearths.

3. Fire-places with hollow backs, hearths and jambs of iron, (described by M. Gauger, in his tract entitled *La Méchanique de Feu,*) for warming the air as it comes into the room.

4. The Holland stoves, with iron doors opening into the room.

5. The German stoves, which have no opening in the room where they are used, but the fire is put in from some other room, or from without.

6. Iron pots, with open charcoal fires, placed in the middle of a room.

1. The first of these methods has generally the conveniency of two warm seats, one in each corner; but they are sometimes too hot to abide in, and, at other times, incommoded with the smoke; there is likewise good room for the cook to move, to hang on pots, &c. Their inconveniences are, that they almost always smoke, if the door be not left open; that they require

a large funnel, and a large funnel carries off a great quantity of air, which occasions what is called a strong draft to the chimney, without which strong draft the smoke would come out of some part or other of so large an opening, so that the door can seldom be shut; and the cold air so nips the backs and heels of those that sit before the fire, that they have no comfort till either screens or settles are provided (at a considerable expense) to keep it off, which both cumber the room, and darken the fire-side. A moderate quantity of wood on the fire, in so large a hearth, seems but little; and, in so strong and cold a draft, warms but little; so that people are continually laying on more. In short, it is next to impossible to warm a room with such a fire-place; and I suppose our ancestors never thought of warming rooms to sit in; all they purposed was, to have a place to make a fire in, by which they might warm themselves when cold.

2. Most of these old-fashioned chimneys in towns and cities have been, of late years, reduced to the second sort mentioned, by building jambs within them, narrowing the hearth, and making a low arch or breast. It is strange, methinks, that though chimneys have been so long in use, their construction should be so little understood till lately, that no workman pretended to make one which should always carry off all smoke, but a chimney-cloth was looked upon as essential to a chimney. This improvement, however, by small openings and low breasts, has been made in our days; and success in the first experiments has brought it into general use in cities, so that almost all new chimneys are now made of that sort, and much fewer bricks will make a stack of chimneys now than formerly. An improvement so lately made may give us room to believe, that still farther improvements may be found to remedy the

inconveniences yet remaining. For these new chimneys, though they keep rooms generally free from smoke, and, the opening being contracted, will allow the door to be shut, yet, the funnel still requiring a considerable quantity of air, it rushes in at every crevice so strongly, as to make a continual whistling or howling; and it is very uncomfortable, as well as dangerous, to sit against any such crevice. Many colds are caught from this cause only, it being safer to sit in the open street; for then the pores do all close together, and the air does not strike so sharply against any particular part of the body.

The Spaniards have a proverbial saying,

"If the wind blows on you through a hole,
Make your will, and take care of your soul."

Women particularly, from this cause, as they sit much in the house, get colds in the head, rheums, and defluctions, which fall into their jaws and gums, and have destroyed early many a fine set of teeth in these northern colonies. Great and bright fires do also very much contribute to damage the eyes, dry and shrivel the skin, and bring on early the appearances of old age. In short, many of the diseases proceeding from colds, as fevers, pleurisies, &c., fatal to very great numbers of people, may be ascribed to strong-drawing chimneys, whereby, in severe weather, a man is scorched before, while he is froze behind.* In the mean time, very little

* As the writer is neither physician nor philosopher, the reader may expect he should justify these his opinions by the authority of some that are so. M. Clare, F. R. S., in his treatise of *The Motion of Fluids*, says, (p. 246, &c.) "And here it may be remarked, that it is more prejudicial to health to sit near a window or door, in a room where there are many candles and a fire, than in a room without; for the consumption of air thereby occasioned, will always be very considerable, and this must necessarily be replaced by cold air from without. Down the chimney can enter none, the stream of warm air always arising therein absolutely forbids it; the supply must therefore come in wherever other openings

is done by these chimneys towards warming the room; for the air round the fire-place, which is warmed by the direct rays from the fire, does not continue in the room, but is continually crowded and gathered into the chimney by the current of cold air coming behind it, and so is presently carried off.

In both these sorts of fire-places, the greatest part of the heat from the fire is lost; for, as fire naturally darts heat every way, the back, the two jambs, and the hearth drink up almost all that is given them, very little being reflected from bodies so dark, porous, and unpolished; and the upright heat, which is by far the greatest, flies directly up the chimney. Thus five sixths at least of the heat (and consequently of the fuel) is wasted, and contributes nothing towards warming the room.

3. To remedy this, the Sieur Gauger gives, in his book, entitled *La Méchanique de Feu*, published in 1709,

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shall be found. If these happen to be small, *let those who sit near them beware;* the smaller the floodgate, the smarter will be the stream. Was a man, even in a sweat, to leap into a cold bath, or jump from his warm bed, in the intensest cold, even in a frost, provided he do not continue over-long therein, and be in health when he does this, we see by experience, that he gets no harm. If he sits a little while against a window, into which a successive current of cold air comes, his pores are closed, and he gets a fever. In the first case, the shock the body endures is general, uniform, and therefore less fierce; in the other, a single part, a neck, or ear perchance, is attacked, and that with the greater violence probably, as it is done by a successive stream of cold air. And the cannon of a battery, pointed against a single part of a bastion, will easier make a breach than were they directed to play singly upon the whole face, and will admit the enemy much sooner into the town."

That warm rooms, and keeping the body warm in winter, are means of preventing such diseases, take the opinion of that learned Italian physician, Antonino Parcio, in the preface to his tract *De Militis Sanitate Tuendâ*, where, speaking of a particular wet and cold winter, remarkable at Venice for its sickliness, he says, "Populares autem pleuritis, quæ Venetiis sæviit mensibus *Dec.*, *Jan.*, *Feb.*, ex cœli aërisque inclementiâ facta est, quod non habeant hypocausta [*stove-rooms*], et quod non soliciti sunt Itali omnes de auribus, temporibus, collo, totoque corpore defendendis ab injurus aëris; et tegmina domorum Veneti disponant parum

seven different constructions of the third sort of chimneys mentioned above, in which there are hollow cavities made by iron plates in the back, jambs, and hearths, through which plates the heat passing warms the air in those cavities, which is continually coming into the room fresh and warm. The invention was very ingenious, and had many conveniences; the room was warmed in all parts, by the air flowing into it through the heated cavities; cold air was prevented rushing through the crevices, the funnel being sufficiently supplied by those cavities; much less fuel would serve, &c. But the first expense, which was very great, the intricacy of the design, and the difficulty of the execution, especially in old chimneys, discouraged the propagation of the invention; so that there are, I suppose, very few such chimneys now in use. The upright heat, too, was almost all lost in these, as in the common chimneys.

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inclinata, ut nives diutius permaneant super tegmina. E contra, Germani, qui experiuntur cœli inclementiam, perdidicere sese defendere ab aëris injuriâ. Tecta construunt multum inclinata, ut decidant nives. Germani abundant lignis, domusque *hypocaustis*; foris autem incedunt pannis, pellibus, gossipio, bene mehercule loricati atque muniti. In Bavariâ interrogabam (curiositate motus videndi Germaniam) quot nam elapsis mensibus pleuritide vel peripneumoniâ fuissent absumti; dicebant vix unus aut alter illis temporibus pleuritide fuit correptus."

The great Dr. Boerhaave, whose authority alone might be sufficient, in his *Aphorisms*, mentions, as one antecedent cause of pleurisies, "a cold air, driven violently through some narrow passage upon the body, overheated by labor or fire."

The eastern physicians agree with the Europeans in this point; witness the Chinese treatise, entitled *Tschang Seng*, that is, *The Art of procuring Health and long Life*, as translated in Père Du Halde's account of China, which has this passage. "As, of all the passions which ruffle us, anger does the most mischief, so of all the malignant affections of the air, a wind that comes through any narrow passage, which is cold and piercing, is most dangerous; and, coming upon us unawares, insinuates itself into the body, often causing grievous diseases. It should therefore be avoided, according to the advice of the ancient proverb, as carefully as the point of an arrow." These mischiefs are avoided by the use of the new-invented fire-places, as will be shown hereafter.

4. The Holland iron stove, which has a flue proceeding from the top, and a small iron door opening into the room, comes next to be considered. Its conveniences are, that it makes a room all over warm; for, the chimney being wholly closed except the flue of the stove, very little air is required to supply that, and therefore not much rushes in at crevices, or at the door when it is opened. Little fuel serves, the heat being almost all saved; for it rays out almost equally from the four sides, the bottom, and the top, into the room, and presently warms the air around it, which, being rarefied, rises to the ceiling, and its place is supplied by the lower air of the room, which flows gradually towards the stove, and is there warmed, and rises in its turn, so that there is a continual circulation till all the air in the room is warmed. The air, too, is gradually changed, by the stove-door's being in the room, through which part of it is continually passing, and that makes these stoves wholesomer, or at least pleasanter than the German stoves, next to be spoken of. But they have these inconveniences. There is no sight of the fire, which is in itself a pleasant thing. One cannot conveniently make any other use of the fire but that of warming the room. When the room is warm, people, not seeing the fire, are apt to forget supplying it with fuel till it is almost out, then, growing cold, a great deal of wood is put in, which soon makes it too hot. The change of air is not carried on quite quick enough; so that, if any smoke or ill smell happens in the room, it is a long time before it is discharged. For these reasons the Holland stove has not obtained much among the English (who love the sight of the fire) unless in some workshops, where people are obliged to sit near windows for the light, and in such places they have been found of good use.

5. The German stove is like a box, one side wanting. It is composed of five iron plates, screwed together, and fixed so as that you may put the fuel into it from another room, or from the outside of the house. It is a kind of oven reversed, its mouth being without, and body within, the room that is to be warmed by it. This invention certainly warms a room very speedily and thoroughly with little fuel; no quantity of cold air comes in at any crevice, because there is no discharge of air which it might supply, there being no passage into the stove from the room. These are its conveniences. Its inconveniences are, that people have not even so much sight or use of the fire as in the Holland stoves, and are, moreover, obliged to breathe the same unchanged air continually, mixed with the breath and perspiration from one another's bodies, which is very disagreeable to those who have not been accustomed to it.

6. Charcoal fires in pots are used chiefly in the shops of handicraftsmen. They warm a room (that is kept close, and has no chimney to carry off the warmed air,) very speedily and uniformly; but, there being no draft to change the air, the sulphurous fumes from the coals (be they ever so well kindled before they are brought in, there will be some,) mix with it, render it disagreeable, hurtful to some constitutions, and sometimes, when the door is long kept shut, produce fatal consequences.

To avoid the several inconveniences, and at the same time retain all the advantages of other fire-places, was contrived the PENNSYLVANIAN FIRE-PLACE, now to be described.

This machine consists of

A bottom plate, (i) (See Plate V.)

A back plate, (ii)

Two side plates, (iii, iii)

Two middle plates, (iv, iv) which, joined together, form a tight box, with winding passages in it for warming the air.

A front plate, (v)

A top plate, (vi)

These are all cast of iron, with mouldings or ledges where the plates come together, to hold them fast, and retain the mortar used for pointing to make tight joints. When the plates are all in their places, a pair of slender rods, with screws, are sufficient to bind the whole very firmly together, as it appears in Figure 2.

There are, moreover, two thin plates of wrought iron, viz. the shutter (vii) and the register (viii); besides the screw-rods, *O*, *P*, all which we shall explain in their order.

(i) The bottom plate, or hearth-piece, is round before, with a rising moulding, that serves as a fender to keep coals and ashes from coming to the floor, &c. It has two ears, *F*, *G*, perforated to receive the screw-rods, *O*, *P*; a long air-hole, *a a*, through which the fresh outward air passes up into the air-box; and three smoke-holes, *B*, *C*, through which the smoke descends and passes away; all represented by dark squares. It has also double ledges to receive between them the bottom edges of the back plate, the two side plates, and the two middle plates. These ledges are about an inch asunder, and about half an inch high; a profile of two of them, joined to a fragment of plate, appears in Figure 3.

(ii) The back plate is without holes, having only a pair of ledges on each side, to receive the back edges of the two

(iii, iii) Side plates; these have each a pair of ledges to receive the side edges of the front plate, and a little

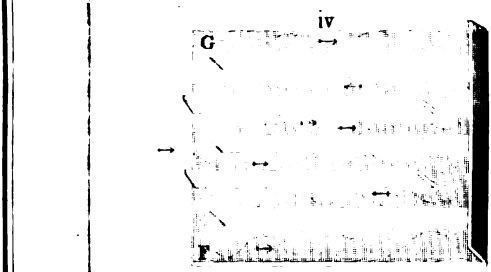
shoulder for it to rest on; also two pair of ledges to receive the side edges of the two middle plates, which form the air-box; and an oblong air-hole near the top, through which is discharged into the room the air warmed in the air-box. Each has also a wing or bracket, *H* and *I*, to keep in falling brands, coals, &c., and a small hole, *Q* and *R*, for the axis of the register to turn in.

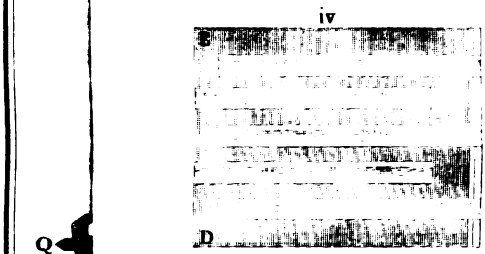
(iv, iv) The air-box is composed of the two middle plates, *D*, *E* and *F*, *G*. The first has five thin ledges or partitions cast on it, two inches deep, the edges of which are received in so many pair of ledges cast in the other. The tops of all the cavities formed by these thin, deep ledges, are also covered by a ledge of the same form and depth, cast with them; so that when the plates are put together, and the joints luted, there is no communication between the air-box and the smoke. In the winding passages of this box, fresh air is warmed as it passes into the room.

(v) The front plate is arched on the under side, and ornamented with foliages, &c.; it has no ledges.

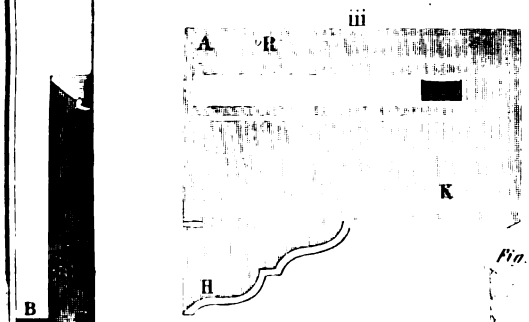
(vi) The top plate has a pair of ears, *M*, *N*, answerable to those in the bottom plate, and perforated for the same purpose; it has also a pair of ledges running round the under side, to receive the top edges of the front, back, and side plates. The air-box does not reach up to the top plate by two inches and a half.

(vii) The shutter is of thin wrought iron and light, of such a length and breadth as to close well the opening of the fire-place. It is used to blow up the fire, and to shut up and secure it at nights. It has two brass knobs for handles, *d*, *d*, and commonly slides up and down in a groove, left, in putting up the fire-place, between the foremost ledge of the side plates, and the face of the front plate; but some choose to set it aside when it is not in use, and apply it on occasion.



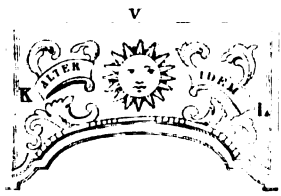


Q



B

Fig. 1.



(viii) The register is also of thin wrought iron. It is placed between the back plate and air-box, and can, by means of the key *S*, be turned on its axis so as to lie in any position between level and upright.

The screw-rods, *O*, *P*, are of wrought iron, about a third of an inch thick, with a button at bottom, and a screw and nut at top, and may be ornamented with two small brasses screwed on above the nuts.

To put this machine to work,

1. A false back of four-inch (or, in shallow small chimneys, two-inch) brick work is to be made in the chimney, four inches or more from the true back; from the top of this false back a closing is to be made over to the breast of the chimney, that no air may pass into the chimney, but what goes under the false back, and up behind it.

2. Some bricks of the hearth are to be taken up, to form a hollow under the bottom plate; across which hollow runs a thin, tight partition, to keep apart the air entering the hollow and the smoke; and is therefore placed between the air-hole and smoke-holes.

3. A passage is made, communicating with the outward air, to introduce that air into the fore part of the hollow under the bottom plate, whence it may rise through the air-hole into the air-box.

4. A passage is made from the back part of the hollow, communicating with the flue behind the false back; through this passage the smoke is to pass.

The fire-place is to be erected upon these hollows, by putting all the plates in their places, and screwing them together.

Its operation may be conceived by observing the plate entitled, *Profile of the Chimney and Fire-place* (See Plate VI.)

M The mantel-piece, or breast of the chimney.

C The funnel.

B The false back and closing.

E True back of the chimney.

T Top of the fire-place.

F The front of it.

A The place where the fire is made.

D The air-box.

K The hole in the side plate, through which the warmed air is discharged out of the air-box into the room.

H The hollow filled with fresh air, entering at the passage *I*, and ascending into the air-box through the air-hole in the bottom plate, near

G The partition in the hollow to keep the air and smoke apart.

P The passage under the false back and part of the hearth for the smoke.

The arrows show the course of the smoke.

The fire being made at *A*, the flame and smoke will ascend and strike the top *T*, which will thereby receive a considerable heat. The smoke, finding no passage upwards, turns over the top of the air-box, and descends between it and the back plate to the holes in the bottom plate, heating, as it passes, both plates of the air-box, and the said back plate; the front plate, bottom and side plates are also all heated at the same time. The smoke proceeds in the passage that leads it under and behind the false back, and so rises into the chimney. The air of the room, warmed behind the back plate, and by the sides, front, and top plates, becoming specifically lighter than the other air in the room, is obliged to rise; but the closure over the fire-place hindering it from going up the chimney, it is forced out into the room, rises by the mantel-piece to the ceiling, and spreads all over the top of the room, whence being crowded down gradually by the stream

of newly-warmed air that follows and rises above it, the whole room becomes in a short time equally warmed.

At the same time, the air, warmed under the bottom plate and in the air-box, rises and comes out of the holes in the side plates, very swiftly, if the door of the room be shut, and joins its current with the stream before mentioned, rising from the side, back, and top plates.

The air that enters the room through the air-box is fresh, though warm; and, computing the swiftness of its motion with the areas of the holes, it is found that near ten barrels of fresh air are hourly introduced by the air-box; and by this means the air in the room is continually changed, and kept at the same time sweet and warm.

It is to be observed, that the entering air will not be warm at first lighting the fire, but heats gradually as the fire increases.

A square opening for a trap-door should be left in the closing of the chimney, for the sweeper to go up; the door may be made of slate or tin, and commonly kept close shut, but so placed as that, turning up against the back of the chimney when open, it closes the vacancy behind the false back, and shoots the soot, that falls in sweeping, out upon the hearth. This trap-door is a very convenient thing.

In rooms where much smoking of tobacco is used, it is also convenient to have a small hole, about five or six inches square, cut near the ceiling through into the funnel; this hole must have a shutter, by which it may be closed or opened at pleasure. When open, there will be a strong draft of air through it into the chimney, which will presently carry off a cloud of smoke, and keep the room clear; if the room be too hot likewise, it will carry off as much of the warm air as you

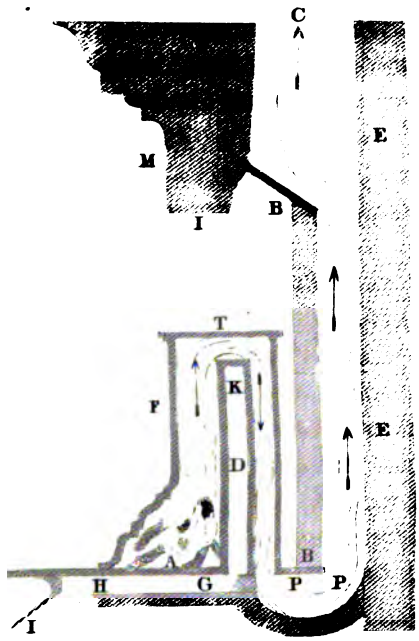
please, and then you may stop it entirely, or in part, as you think fit. By this means it is, that the tobacco smoke does not descend among the heads of the company near the fire, as it must do before it can get into common chimneys.

The Manner of using this Fire-place.

Your cord-wood must be cut into three lengths; or else a short piece, fit for the fire-place, cut off, and the longer left for the kitchen or other fires. Dry hickory, or ash, or any woods that burn with a clear flame, are rather to be chosen, because such are less apt to foul the smoke passages with soot; and flame communicates with its light, as well as by contact, greater heat to the plates and room. But, where more ordinary wood is used, half a dry fagot of brushwood, burnt at the first making the fire in the morning, is very advantageous, as it immediately, by its sudden blaze, heats the plates, and warms the room (which with bad wood slowly kindling would not be done so soon), and at the same time by the length of its flame, turning in the passages, consumes and cleanses away the soot that such bad, smoky wood had produced therein the preceding day, and so keeps them always free and clean. When you have laid a little back log, and placed your billets on small dogs, as in common chimneys, and put some fire to them, then slide down your shutter as low as the dogs, and the opening being by that means contracted, the air rushes in briskly, and presently blows up the flames. When the fire is sufficiently kindled, slide it up again.* In some of these fire-places there

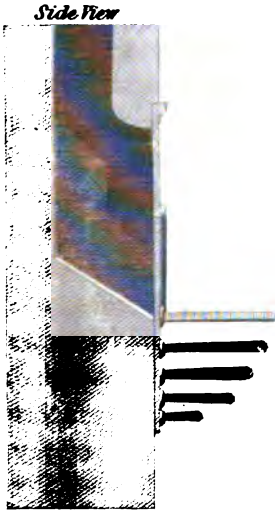
* The shutter is slid up and down in this manner, only in those fire-places which are so made, as that the distance between the top of the arched opening and the bottom plate is the same as the distance between

PROFILE OF THE PENNSYLVANIA CHIMNEY AND FIRE-PLACE

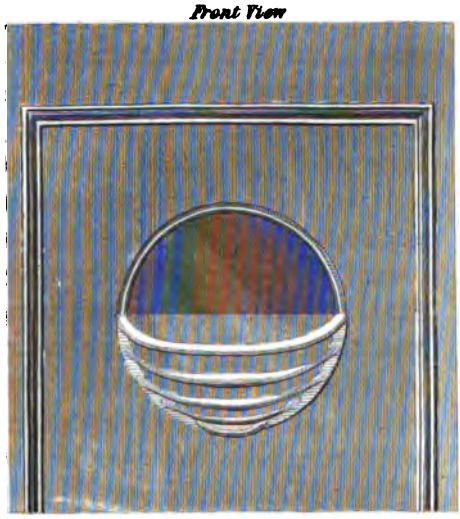


STAFFORDSHIRE FIRE-PLACE. *Page 531.*

Side View



Front View



Boston. Published by Hilliard, Gray & C.°

is a little six inch square trap-door of thin wrought iron or brass, covering a hole of like dimensions near the fore part of the bottom plate, which being by a ring lifted up towards the fire, about an inch, where it will be retained by two springing sides fixed to it perpendicularly (see Plate V., Fig. 4), the air rushes in from the hollow under the bottom plate, and blows the fire. Where this is used, the shutter serves only to close the fire at nights. The more forward you can make your fire on the hearth-plate, not to be incommoded by the smoke, the sooner and more will the room be warmed. At night, when you go to bed, cover the coals or brands with ashes as usual; then take away the dogs, and slide down the shutter close to the bottom plate, sweeping a little ashes against it, that no air may pass under it; then turn the register, so as very near to stop the flue behind. If no smoke then comes out at crevices into the room, it is right; if any smoke is perceived to come out, move the register, so as to give a little draft, and it will go the right way. Thus the room will be kept warm all night; for, the chimney being almost entirely stopt, very little cold air, if any, will enter the room at any crevice. When you come to rekindle the fire in the morning, turn open the register before you lift up the slider, otherwise, if there be any smoke in the fire-place, it will come out into the room. By the same use of the shutter and register, a blazing fire may be presently stifled, as well as secured, when you have occasion to leave it for any time; and at your return you will find the brands warm, and ready for a speedy rekindling. The shutter alone will not stifle a fire, for

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it and the top plate. Where the arch is higher, as it is in the draft annexed (which is agreeable to the last improvements), the shutter is set by, and applied occasionally; because, if it were made deep enough to close the whole opening when slid down, it would hide part of it when up.

it cannot well be made to fit so exactly but that air will enter, and that in a violent stream, so as to blow up and keep alive the flames, and consume the wood, if the draft be not checked by turning the register to shut the flue behind. The register has also two other uses. If you observe the draft of air into your fire-place to be stronger than is necessary (as in extreme cold weather it often is), so that the wood is consumed faster than usual; in that case, a quarter, half, or two-thirds turn of the register will check the violence of the draft, and let your fire burn with the moderation you desire; and at the same time both the fire-place and the room will be the warmer, because less cold air will enter and pass through them. And, if the chimney should happen to take fire (which indeed there is very little danger of, if the preceding direction be observed in making fires, and it be well swept once a year; for, much less wood being burnt, less soot is proportionably made; and, the fuel being soon blown into flame by the shutter, or the trap-door bellows, there is consequently less smoke from the fuel to make soot; then, though the funnel should be foul, yet the sparks have such a crooked, up and down, round-about way to go, that they are out before they get at it;) I say, if ever it should be on fire, a turn of the register shuts all close, and prevents any air going into the chimney, and so the fire may easily be stifled and mastered.

The Advantages of this Fire-place.

Its advantages above the common fire-places are,

1. That your whole room is equally warmed, so that people need not crowd so close round the fire, but may sit near the window, and have the benefit of the light for reading, writing, needlework, &c. They may sit

with comfort in any part of the room, which is a very considerable advantage in a large family, where there must often be two fires kept, because all cannot conveniently come at one.

2. If you sit near the fire, you have not that cold draft of uncomfortable air nipping your back and heels, as when before common fires, by which many catch cold, being scorched before, and, as it were, froze behind.

3. If you sit against a crevice, there is not that sharp draft of cold air playing on you, as in rooms where there are fires in the common way; by which many catch cold, whence proceed coughs,* catarrhs, tooth-aches, fevers, pleurisies, and many other diseases.

4. In case of sickness, they make most excellent nursing-rooms; as they constantly supply a sufficiency of fresh air, so warmed at the same time as to be no way inconvenient or dangerous. A small one does well in a chamber; and, the chimneys being fitted for it, it may be removed from one room to another, as occasion requires, and fixed in half an hour. The equal temper, too, and warmth of the air of the room, is thought to be particularly advantageous in some distempers; for it was observed in the winters of 1730 and 1736, when the small-pox spread in Pennsylvania, that very few children of the Germans died of that distemper in proportion to those of the English; which was ascribed, by some, to the warmth and equal temper of air in their stove-rooms, which made the disease as favorable as

* My Lord Molesworth, in his account of Denmark, says, "That few or none of the people there are troubled with coughs, catarrhs, consumptions, or such like diseases of the lungs; so that in the midst of winter in the churches, which are very much frequented, there is no noise to interrupt the attention due to the preacher. I am persuaded," says he, "their *warm stoves* contribute to their freedom from these kinds of maladies." p. 91.

it commonly is in the West Indies. But this conjecture we submit to the judgment of physicians.

5. In common chimneys, the strongest heat from the fire, which is upwards, goes directly up the chimney, and is lost; and there is such a strong draft into the chimney, that not only the upright heat, but also the back, sides, and downward heats are carried up the chimney by that draft of air; and the warmth given before the fire, by the rays that strike out towards the room, is continually driven back, crowded into the chimney, and carried up by the same draft of air. But here the upright heat strikes and heats the top plate, which warms the air above it, and that comes into the room. The heat likewise, which the fire communicates to the sides, back, bottom, and air-box, is all brought into the room; for you will find a constant current of warm air coming out of the chimney corner into the room. Hold a candle just under the mantel-piece, or breast of your chimney, and you will see the flame bent outwards; by laying a piece of smoking paper on the hearth, on either side, you may see how the current of air moves, and where it tends, for it will turn and carry the smoke with it.

6. Thus, as very little of the heat is lost, when this fire-place is used, *much less wood** will serve you, which is a considerable advantage where wood is dear.

* People, who have used these fire-places, differ much in their accounts of the wood saved by them. Some say five sixths, others three fourths, and others much less. This is owing to the great difference there was in their former fires; some (according to the different circumstances of their rooms and chimneys) having been used to make very large, others middling, and others, of a more sparing temper, very small ones; while in these fire-places (their size and draft being nearly the same) the consumption is more equal. I suppose, taking a number of families together, that two thirds, or half the wood, at least, is saved. My common room, I know, is made twice as warm as it used to be, with a quarter of the wood I formerly consumed there.

7. When you burn candles near this fire-place, you will find that the flame burns quite upright, and does not blare and run the tallow down, by drawing towards the chimney, as against common fires.

8. This fire-place cures most smoky chimneys, and thereby preserves both the eyes and furniture.

9. It prevents the fouling of chimneys; much of the lint and dust that contributes to foul a chimney being, by the low arch, obliged to pass through the flame, where it is consumed. Then, less wood being burnt, there is less smoke made. Again, the shutter, or trap-bellows, soon blowing the wood into a flame, the same wood does not yield so much smoke as if burnt in a common chimney; for, as soon as flame begins, smoke in proportion ceases.

10. And, if a chimney should be foul, it is much less likely to take fire. If it should take fire, it is easily stifled and extinguished.

11. A fire may be very speedily made in this fire-place by the help of the shutter, or trap-bellows, as aforesaid.

12. A fire may be soon extinguished by closing it with the shutter before, and turning the register behind, which will stifle it, and the brands will remain ready to rekindle.

13. The room being once warm, the warmth may be retained in it all night.

14. And lastly, the fire is so secured at night, that not one spark can fly out into the room to do damage.

With all these conveniences, you do not lose the pleasing sight nor use of the fire, as in the Dutch stoves, but may boil the tea-kettle, warm the flat-irons, heat heaters, keep warm a dish of victuals by setting it on the top, &c.

Objections answered.

There are some objections commonly made by people that are unacquainted with these fire-places, which it may not be amiss to endeavour to remove, as they arise from prejudices which might otherwise obstruct, in some degree, the general use of this beneficial machine. We frequently hear it said, *They are of the nature of Dutch stoves; stoves have an unpleasant smell; stoves are unwholesome; and warm rooms make people tender, and apt to catch cold.* As to the first, that they are of the nature of Dutch stoves, the description of those stoves, in the beginning of this paper, compared with that of these machines, shows that there is a most material difference, and that these have vastly the advantage, if it were only in the single article of the admission and circulation of the fresh air. But it must be allowed there may have been some cause to complain of the offensive smell of iron stoves. This smell, however, never proceeded from the iron itself, which, in its nature, whether hot or cold, is one of the sweetest of metals, but from the general uncleanly manner of using those stoves. If they are kept clean, they are as sweet as an ironing-box, which, though ever so hot, never offends the smell of the nicest lady; but it is common to let them be greased, by setting candlesticks on them, or otherwise; to rub greasy hands on them; and, above all, to spit upon them, to try how hot they are, which is an inconsiderate, filthy, unmannerly custom; for the slimy matter of spittle, drying on, burns and fumes when the stove is hot, as well as the grease, and smells most nauseously, which makes such close stove-rooms, where there is no draft to carry off those filthy vapors, almost intolerable to those that are not

from their infancy accustomed to them. At the same time nothing is more easy than to keep them clean; for, when by any accident they happen to be fouled, a lie made of ashes and water, with a brush, will scour them perfectly; as will also a little strong soft soap and water.

That hot iron of itself gives no offensive smell, those know very well who have (as the writer of this has) been present at a furnace when the workmen were pouring out the flowing metal to cast large plates, and not the least smell of it to be perceived. That hot iron does not, like lead, brass, and some other metals, give out unwholesome vapors, is plain from the general health and strength of those who constantly work in iron, as furnace-men, forge-men, and smiths; that it is in its nature a metal perfectly wholesome to the body of man, is known from the beneficial use of chalybeate or iron-mine waters; from the good done by taking steel filings in several disorders; and that even the smithy water, in which hot irons are quenched, is found advantageous to the human constitution. The ingenious and learned Dr. Desaguliers, to whose instructive writings the contriver of this machine acknowledges himself much indebted, relates an experiment he made, to try whether heated iron would yield unwholesome vapors. He took a cube of iron, and, having given it a very great heat, he fixed it so to a receiver, exhausted by the air-pump, that all the air, rushing in to fill the receiver, should first pass through a hole in the hot iron. He then put a small bird into the receiver, who breathed that air without any inconvenience, or suffering the least disorder. But the same experiment being made with a cube of hot brass, a bird put into that air died in a few minutes. Brass, indeed, stinks even when cold, and much more when hot; lead, too,

when hot, yields a very unwholesome steam; but iron is always sweet, and every way taken is wholesome and friendly to the human body, except in weapons.

That warmed rooms make people tender and apt to catch cold, is a mistake as great as it is (among the English) general. We have seen in the preceding pages how the common rooms are apt to give colds; but the writer of this paper may affirm from his own experience, and that of his family and friends, who have used warm rooms for these four winters past, that by the use of such rooms, people are rendered *less liable* to take cold, and, indeed, *actually hardened*. If sitting warm in a room made one subject to take cold on going out, lying warm in bed should, by a parity of reason, produce the same effect when we rise. Yet we find we can leap out of the warmest bed naked, in the coldest morning, without any such danger; and in the same manner out of warm clothes into a cold bed. The reason is, that in these cases the pores all close at once, the cold is shut out, and the heat within augmented, as we soon after feel by the glowing of the flesh and skin. Thus, no one was ever known to catch cold by the use of the cold bath; and are not cold baths allowed to harden the bodies of those that use them? Are they not therefore frequently prescribed to the tenderest constitutions? Now, every time you go out of a warm room into the cold, freezing air, you do as it were plunge into a cold bath, and the effect is in proportion the same; for (though perhaps you may feel somewhat chilly at first) you find in a little time your bodies hardened and strengthened, your blood is driven round with a brisker circulation, and a comfortable, steady, uniform inward warmth succeeds that equal outward warmth you first received in the room. Farther to confirm this assertion, we instance the Swedes, the

Danes, and the Russians; these nations are said to live in rooms, compared to ours, as hot as ovens;* yet where are the hardy soldiers, though bred in their boasted cool houses, that can, like these people, bear the fatigues of a winter campaign in so severe a climate, march whole days to the neck in snow, and at night intrench in ice, as they do?

The mentioning of those northern nations puts me in mind of a considerable *public advantage* that may arise from the general use of these fire-places. It is observable, that, though those countries have been well inhabited for many ages, wood is still their fuel, and yet at no very great price; which could not have been, if they had not universally used stoves, but consumed it as we do in great quantities, by open fires. By the help of this saving invention our wood may grow as fast as we consume it, and our posterity may warm themselves at a moderate rate, without being obliged to fetch their fuel over the Atlantic; as, if pit-coal should not be here discovered (which is an uncertainty) they must necessarily do.

We leave it to the *political arithmetician* to compute how much money will be saved to a country, by its spending two thirds less of fuel; how much labor saved

* Mr. Boyle, in his experiments and observations upon cold, *Shaw's Abridgment*, Vol. I. p. 684, says, "It is remarkable, that, while the cold has strange and tragical effects at Moscow and elsewhere, the Russians and Livonians should be exempt from them, who accustom themselves to pass immediately from a great degree of heat, to as great a one of cold, without receiving any visible prejudice thereby. I remember being told by a person of unquestionable credit, that it was a common practice among them, to go from a hot stove into cold water; the same was also affirmed to me by another who resided at Moscow. This tradition is likewise abundantly confirmed by Olearius." "It is a surprising thing," says he, "to see how far the Russians can endure heat; and how, when it makes them ready to faint, they can go out of their stoves, stark naked, both men and women, and throw themselves into cold water, and even in winter wallow in the snow."

in cutting and carriage of it; how much more land may be cleared by cultivation; how great the profit by the additional quantity of work done, in those trades particularly that do not exercise the body so much, but that the workfolks are obliged to run frequently to the fire to warm themselves; and to physicians to say, how much healthier thick-built towns and cities will be, now half suffocated with sulphury smoke, when so much less of that smoke shall be made, and the air breathed by the inhabitants be consequently so much purer. These things it will suffice just to have mentioned; let us proceed to give some necessary directions to the workman, who is to fix or set up these fire-places.

Directions to the Bricklayer.

The chimney being first well swept and cleansed from soot, &c., lay the bottom plate down on the hearth, in the place where the fire-place is to stand, which may be as forward as the hearth will allow. Chalk a line from one of its back corners round the plate to the other corner, that you may afterwards know its place when you come to fix it; and from those corners, two parallel lines to the back of the chimney; make marks also on each side, that you may know where the partition is to stand, which is to prevent any communication between the air and smoke. Then, removing the plate, make a hollow under it and beyond it, by taking up as many of the bricks or tiles as you can, within your chalked lines, quite to the chimney-back. Dig out six or eight inches deep of the earth or rubbish, all the breadth and length of your hollow; then make a passage of four inches square (if the place will allow so much) leading from the hollow to some place communicating with the outer air; by *outer air* we mean air

without the room you intend to warm. This passage may be made to enter your hollow on either side, or in the fore part, just as you find most convenient, the circumstances of your chimney considered. If the fire-place is to be put up in a chamber, you may have this communication of outer air from the staircase; or sometimes more easily from between the chamber floor and the ceiling of the lower room, making only a small hole in the wall of the house entering the space betwixt those two joists with which your air-passage in the hearth communicates. If this air-passage be so situated as that mice may enter it, and nestle in the hollow, a little grate of wire will keep them out. This passage being made, and, if it runs under any part of the hearth, tiled over securely, you may proceed to raise your false back. This may be of four inches or two inches thickness, as you have room; but let it stand at least four inches from the true chimney back. In narrow chimneys this false back runs from jamb to jamb; but in large, old-fashioned chimneys, you need not make it wider than the back of the fire-place. To begin it, you may form an arch nearly flat, of three bricks end to end, over the hollow, to leave a passage the breadth of the iron fire-place, and five or six inches deep, rounding at bottom, for the smoke to turn and pass under the false back, and so behind it up the chimney. The false back is to rise till it is as high as the breast of the chimney, and then to close over to the breast;* always observing, if there is a wooden mantel-tree, to close above it. If there is no wood in the breast, you may arch over and close even with the lower part of the breast. By this closing the chimney is made tight,

* See page 49, where the trap-door is described, that ought to be in this closing.

that no air or smoke may pass up it, without going under the false back. Then from side to side of your hollow, against the marks you made with chalk, raise a tight partition, brick-on-edge, to separate the air from the smoke, bevelling away to half an inch the brick that comes just under the air-hole, that the air may have a free passage up into the air-box. Lastly, close the hearth over that part of the hollow that is between the false back and the place of the bottom plate, coming about half an inch under the plate, which piece of hollow hearth may be supported by a bit or two of old iron hoop; then is your chimney fitted to receive the fire-place.

To set it, lay first a little bed of mortar all round the edges of the hollow, and over the top of the partition; then lay down your bottom plate in its place (with the rods in it) and tread it till it lies firm. Then put a little fine mortar (made of loam and lime, with a little hair,) into its joints, and set in your back plate, leaning it for the present against the false back; then set in your air-box, with a little mortar in its joints; then put in the two sides, closing them up against the air-box, with mortar in their grooves, and fixing at the same time your register; then bring up your back to its place, with mortar in its grooves, and that will bind the sides together. Then put in your front plate, placing it as far back in the groove as you can, to leave room for the sliding plate; then lay on your top plate, with mortar in its grooves also, screwing the whole firmly together by means of the rods. The capital letters, *A*, *B*, *D*, *E*, &c. in the annexed cut [Plate VI.], show the corresponding parts of the several plates. Lastly, the joints being pointed all round on the outside, the fire-place is fit for use.

When you make your first fire in it, perhaps, if the

chimney be thoroughly cold, it may not draw, the work too being all cold and damp. In such case, put first a few shovels of hot coals in the fire-place, then lift up the chimney sweeper's trap-door, and putting in a sheet or two of flaming paper, shut it again, which will set the chimney a drawing immediately, and, when once it is filled with a column of warm air, it will draw strongly and continually.

The drying of the mortar and work by the first fire may smell unpleasantly, but that will soon be over.

In some shallow chimneys, to make more room for the false back and its flue, four inches or more of the chimney-back may be picked away.

Let the room be made as tight as conveniently it may be; so will the outer air, that must come in to supply the room and draft of the fire, be all obliged to enter through the passage under the bottom plate, and up through the air-box, by which means it will not come cold to your backs, but be warmed as it comes in, and mixed with the warm air round the fire-place, before it spreads into the room.

But, as a great quantity of cold air, in extreme cold weather, especially, will presently enter a room if the door be carelessly left open, it is good to have some contrivance to shut it, either by means of screw hinges, a spring, or a pulley.

When the pointing in the joints is all dry and hard, get some powder of black lead (broken bits of black lead crucibles from the silversmiths, pounded fine, will do), and mixing it with a little rum and water, lay it on, when the plates are warm, with a hard brush, over the top and front plates, part of the side and bottom plates, and over all the pointing; and, as it dries, rub it to a gloss with the same brush, so the joints will not be discerned, but it will look all of a piece, and shine like

new iron. And, the false back being plastered and whitewashed, and the hearth reddened, the whole will make a pretty appearance. Before the black lead is laid on, it would not be amiss to wash the plates with strong lie and a brush, or soap and water, to cleanse them from any spots of grease or filth that may be on them. If any grease should afterwards come on them, a little wet ashes will get it out.

If it be well set up, and in a tolerably good chimney, smoke will draw in from as far as the fore part of the bottom plate, as you may try by a bit of burning paper.

People are at first apt to make their rooms too warm, not imagining how little a fire will be sufficient. When the plates are no hotter than that one may just bear the hand on them, the room will generally be as warm as you desire it.*

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* When this pamphlet was first printed, a copy of it was sent by Mr. Cadwallader Colden to the celebrated Gronovius, with a letter from which the following is an extract.

"I send with this a curious and new invention for warming a room with a small fire, and more effectually than can be done by a large fire, in the common method, and is free of the inconveniences which attend the Dutch and German stoves, because by this contrivance there is a continual supply of fresh warm air. It may be particularly useful to you and Dr. Linnæus, by preserving your health while it keeps you warm at your studies. It is the invention of Mr. Benjamin Franklin of Philadelphia, the printer of it, a very ingenious man."

To this passage Gronovius replied as follows, in a letter dated at Leyden, July 9th, 1745.

"I am very much obliged to you for Mr. Franklin's book, which I don't doubt the next letter shall bring to you translated into Dutch.' —EDITOR.

TO CADWALLADER COLDEN.

On Perspiration and Absorption. — Motion of the Blood in the Heart.

Philadelphia, 15 August, 1745.

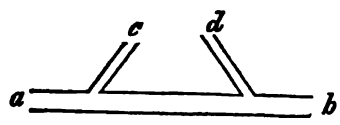
SIR,

I received your favor of the 20th past, with your medical piece enclosed, the reading of which gave me a great deal of pleasure. I showed it to our friend Mr. Bartram, who carried it home, and, as he since tells me, is taking a copy of it. His keeping of it for that end has prevented my showing it to any other gentleman as you desired, and hitherto prevented my writing to you upon it, as I intended. But, lest you should conclude me the very worst correspondent in the world, I shall delay no longer giving you some thoughts, that occurred to me in reading of it, choosing rather to be blamed for not writing to the purpose, than for not writing at all.

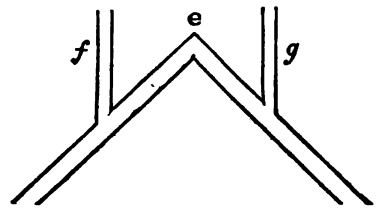
I am extremely pleased with your doctrine of the *absorbent vessels* intermixed with the perspiratory ducts, both on the external and internal superficies of the body. After I had read Sanctorius, I imagined a constant stream of the perspirable matter issuing at *every* pore in the skin. But then I was puzzled to account for the effects of mercurial unctions for the strangury, sometimes occasioned by an outward application of the flies, and the like; since whatever virtue or quality might be in a medicine laid upon the skin, if it would enter the body, it must go against wind and tide, as one may say. Dr. Hales helped me a little, when he informed me, in his *Vegetable Statics*, that the body is not always in a perspirable, but sometimes in an *imbibing state*, as he expresses it, and will at times actually grow heavier

by being exposed to moist air. But this did not quite remove my difficulty; since, as these fits of imbibing did not appear to be regular or frequent, a blistering plaster might lie on the body a week, or a mercurial unguent be used a month, to no purpose, if the body should so long continue in a perspirable state. Your doctrine, which was quite new to me, makes all easy; since the body may perspire and absorb at the same time, through the different ducts destined to those different ends.

I must own, however, that I have one objection to the explanation you give of the operation of these absorbents. That they should communicate with the veins, and the perspirants with the arteries only, seems natural enough; but, as all fluids by the hydrostatical law pass equally in all directions, I question whether the *mere direction* of one of those minute vessels, where it joins with a vein or artery, *with* or *against* the stream of blood in the larger vessel, would be sufficient to produce such contrary effects as *perspiring* and *absorbing*. If it would, both perspirants and absorbents might proceed from the arteries only, or from the veins only, or from both indifferently; as, by the figure in the margin, whether the vessel *a b* is an artery or a vein, if the stream moves from *a* to *b*, the minute communicating vessel *c* shall be a perspirant, and *d* an absorbent; and the contrary, if it moves from *b* to *a*. Yet I cannot say I am certain the mere direction of the vessels will have no effect; I only suspect it, and am making a little machine to try an experiment with, for satisfaction.



It is a siphon made of two large joints of Carolina



cane united at *e*, into which two small glass tubes, *f* and *g*, are to be inserted, one on the descending, and the other on the ascending side. I propose to fill the siphon and the two glass tubes with water, and, when it is playing, unstop at the same instant the tops of both glass tubes, observing in which the water sinks fastest. You shall know the success. I conceive the pressure of the atmosphere on the apertures of the two glass tubes to be no way different from the pressure of the same on the mouths of the perspirants and absorbents, and if the water sinks equally in the two tubes, notwithstanding the direction of one against and the other with the stream, I shall be ready to think we must look out for another solution. You will say, perhaps, that it will then be time enough when the experiment is tried, and succeeds as I suspect; yet I cannot forbear attempting at one beforehand, while some thoughts are present in my mind. If a new solution should be found necessary, this may be ready for consideration.

I do not remember, that any anatomist, that has fallen in my way, has assigned any other cause of the motion of the blood through its whole circle, than the contractile force of the heart, by which that fluid is driven with violence into the arteries, and so continually propelled by repetitions of the same force, till it arrives at the heart again. May we for our present purpose suppose another cause producing half the effect, and say that the ventricles of the heart, like syringes, *draw* when they dilate, as well as force when they contract? That this is not unlikely, may be judged from the

valves nature has placed in the arteries, to prevent the drawing back of the blood in those vessels when the heart dilates, while no such obstacles prevent its sucking (to use the vulgar expression) from the veins. If this be allowed, and the insertion of the absorbents into the veins and of the perspirants into the arteries be agreed to, it will be of no importance in what direction they are inserted. For, as the branches of the arteries are continually lessening in their diameters, and the motion of the blood decreasing by means of the increased resistance, there must, as more is constantly pressed on behind, arise a kind of *crowding* in the extremities of those vessels, which will naturally *force out* what is contained in the perspirants that communicate with them. This lessens the quantity of blood, so that the heart cannot receive again by the veins all it had discharged into the arteries, which occasions it to draw strongly upon the absorbents, that communicate with them. And thus the body is continually perspiring and imbibing. Hence after long fasting the body is more liable to receive infection from bad air, and food, before it is sufficiently chylified, is drawn crude into the blood by the absorbents that open into the bowels.

To confirm this position, that the heart *draws*, as well as *drives* the blood, let me add this particular. If you sit or lean long, in such a manner as to compress the principal artery that supplies a limb with blood, so that it does not furnish a due quantity, you will be sensible of a pricking pain in the extremities like that of a thousand needles; and the veins, which used to raise your skin in ridges, will be (with the skin) sunk into channels; the blood being drawn out of them, and their sides pressed so closely together that it is with difficulty and slowly that the blood afterwards enters them, when the compressed artery is relieved. If the blood

was not drawn by the heart, the compression of an artery would not empty a vein, and I conjecture that the pricking pain is occasioned by the sides of the small vessels being pressed together.

I am not without apprehension, that this hypothesis is either not new, or, if it is new, not good for any thing. It may, however, in this letter, with the enclosed paper on a kindred subject, serve to show the great confidence I place in your candor, since to you I so freely hazard myself *(ultra crepidam)* in meddling with matters directly pertaining to your profession, and entirely out of the way of my own. If you give yourself the trouble of reading them, it is all I can modestly expect. Your silence about them afterwards will be sufficient to convince me, that I am in the wrong, and that I ought to study the sciences I dabble in, before I presume to set pen to paper. I will endeavour, however, to make you some amends by procuring you from better judges some better remarks on the rest of your piece, and shall observe your caution not to let them know from whom I had it.

The piece on Fluxions I purpose shortly to read again, and that on the several species of matter, when you shall have what little I shall be able to say about them.

The members of our Society here are very idle gentlemen. They will take no pains. I must, I believe, alter the scheme and proceed with the papers I have, and may receive, in the manner you advise in one of your former letters. The mention of your former letters puts me in mind how much I am in arrear with you. Like some honest insolvent debtors, I must resolve to pay ready money for what I have hereafter, and discharge the old debt by little and little as I am able.

The impertinence of these mosquitos to me (now I am in the humor of writing) prevents a great deal of mine to you, so that, for once, they are of some use in the world. I am, Sir,

Your most humble servant,

B. FRANKLIN.

TO CADWALLADER COLDEN.

On the Circulation of the Blood. — Proposal for publishing a Philosophical Miscellany.

Philadelphia, 28 November, 1745.

SIR,

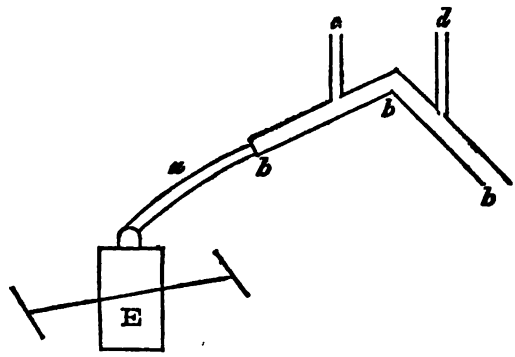
I shall be very willing and ready, when you think proper to publish your piece on gravitation, to print it at my own expense and risk. If I can be the means of communicating any thing valuable to the world, I do not always think of gaining, nor even of saving, by my business; but a piece of that kind, as it must excite the curiosity of all the learned, can hardly fail of bearing its own expense.

I must not pretend to dispute with you on any part of the animal economy. You are quite too strong for me. I shall just mention two or three little things, that I am not quite clear in.

If there is no contrivance in the frame of the auricles or ventricles of the heart, by which they dilate themselves, I cannot conceive how they are dilated. It is said, by the force of the venal blood rushing into them. But, if that blood has no force which was not first given to it by the contraction of the heart, how can it (diminished as it must be by the resisting friction of the vessels it has passed through) be strong enough to over-

come that contraction? Your doctrine of fermentation in the capillaries helps me a little; for, if the returning blood be rarefied by the fermentation, its motion must be increased; but, as it seems to me that it must by its expansion resist the arterial blood behind it, as much as it accelerates the venal blood before it, I am still somewhat unsatisfied. I have heard or read somewhere, too, that the hearts of some animals continue to contract and dilate, or to beat, as it is commonly expressed, after they are separated from the other vessels, and taken out of the body. If this be true, their dilation is not caused by the force of the returning blood.

I should be glad to satisfy myself, too, whether the blood is always quicker in motion, when the pulse beats quicker. Perhaps more blood is driven forward by one strong, deep stroke, than by two that are weak and light; as a man may breathe more air by one long common respiration, when in health, than by two quick, short ones in a fever. I applied the siphon I mentioned to you in a former letter to the pipe of a water-engine.



E is the engine; *a*, its pipe; *b b b*, the siphon; *c* and *d*, the two glass pipes communicating with the siphon. Upon working the engine, the water flowed through the siphon, and the glass tube *c*; but none was discharged through *d*. When I stopped with my finger

the siphon, the water issued at both glass tubes, with equal force, and on only half stopping the end of the siphon, it did the same. I imagine the sudden bending of the siphon gives such a resistance to the stream, as to occasion its issuing out of the glass tube *c*. But I intend to try a farther experiment, of which I shall give you an account.

I am now determined to publish an *American Philosophical Miscellany*, monthly or quarterly. I shall begin with next January, and proceed as I find encouragement and assistance. As I purpose to take the compiling wholly upon myself, the reputation of no gentleman or society will be affected by what I insert of another's; and that perhaps will make them more free to communicate. Their names shall be published or concealed, as they think proper, and care taken to do exact justice to matters of invention, &c. I shall be glad of your advice in any particulars that occurred to you in thinking of this scheme; for, as you first proposed it to me, I doubt not but you have well considered it.*

I have not the original of Dr. Mitchell's tract on the Yellow Fever.† Mine is a copy I had taken, with his leave, when here. Mr. Evans will make a copy of it for you.

I hope it will be confirmed by future experiment, that the *yaws* are to be cured by tar-water. The case you relate to Dr. Mitchell gives great hopes of it, and should

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* It does not appear that this scheme was ever carried into execution. — EDITOR.

† Dr. John Mitchell was a learned physician and botanist, and Fellow of the Royal Society. He was a native of England, but came over and established himself in Virginia. Dr. Miller says, that "he wrote ably on the yellow fever, as it appeared in Virginia in 1742; and that his instructive manuscripts on this subject fell into the hands of Dr. Franklin, by whom they were communicated to Dr. Rush." — MILLER'S *Retrospect*, Vol. I. p. 318. — EDITOR.

be published, to induce people to make trials. For, though it should not always succeed, I suppose there is no danger of its doing any harm.

As to your pieces on Fluxions and the different species of matter, it is not owing to reservedness that I have not yet sent you my thoughts; but because I cannot please myself with them, having had no leisure yet to digest them. If I was clear, that you are anywhere mistaken, I would tell you so, and give my reasons with all freedom, as believing nothing I could do would be more obliging to you. I am persuaded you think, as I do, that he who removes a prejudice, or an error, from our minds, contributes to their beauty, as he would do to that of our faces, who should clear them of a wart or a wen.

I have a friend gone to New York with a view of settling there, if he can meet with encouragement. It is Dr. John Bard, whom I esteem an ingenious physician and surgeon, and a discreet, worthy, and honest man. If, upon conversation with him, you find this character just, I doubt not but you will afford him your advice and countenance, which will be of great service to him in a place where he is entirely a stranger, and very much oblige, Sir,

Your most humble servant,*

B. FRANKLIN.

P. S. I shall forward your letter to Dr. Mitchell. Thank you for leaving it open for my perusal.

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* This introduction was the origin of a long and intimate friendship between Dr. Bard and Mr. Colden. For his professional skill, his active public spirit, and his private worth, no man of his time was more highly valued than Dr. John Bard. His talents and his virtues were inherited by his son, Dr. Samuel Bard, of whom a very interesting memoir has been published by Professor Mc Vickar. In that work, speaking of Dr. John Bard, the father, Professor Mc Vickar says; "He was a man who

TO CADWALLADER COLDEN.

Conjecture as to the Cause why Ships in crossing the Atlantic have longer Passages in sailing westward than in sailing eastward.

[Date uncertain.]

SIR,

I received yours, with others enclosed for Mr. Bartram and Mr. Armit, to which I suppose the enclosed are answers. The persons who brought yours said he would call for answers, but did not; or, if he did, I did not see him.

I understand Parker* has begun upon your piece. A long sitting of our Assembly has hitherto hindered me from beginning the *Miscellany*. I shall write to Dr. Gronovius as you desire.

I wish I had mathematics enough to satisfy myself, whether the much shorter voyages made by ships bound hence to England, than by those from England hither, are not in some degree owing to the diurnal motion of the earth; and if so, in what degree. It is a notion that has lately entered my mind; I know not if ever any other's. Ships in a calm at the equator move with the sea fifteen miles per minute; at our Cape suppose twelve miles per minute; in the British Channel suppose

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will not be quickly forgotten where he was once known; in whom native taste and talent made up so fully for the deficiencies of early education, that he was the intimate friend and favorite companion of the few literary men of his period; and in whose manners and conversation, frankness and urbanity were so happily blended, that, wherever he went, he softened hostility, conciliated good-will, and turned accidental acquaintance into personal and warm friendship." Several admirable letters from him to his son are printed in Professor M^c^Vickar's Memoir; in which are also contained many particulars respecting his intercourse with Franklin. — EDITOR.

* A printer in New York. — EDITOR.

ten miles per minute. Here is a difference of two miles' velocity per minute between Cape Henlopen and the Lizard. No small matter in so weighty a body as a laden ship, swimming in a fluid. How is this velocity lost in the voyage thither, if not by the resistance of the water? And if so, then the water, which resisted in part, must have given way in part to the ship, from time to time, as she proceeded continually out of parallels of latitude where the earth's motion or rotation was quicker, into others where it was slower. And thus, as her velocity tends eastward with the earth's motion, she perhaps makes her easting sooner. Suppose a vessel lying still in a calm at our Cape could be taken up, and the same instant set down in an equal calm in the English Channel; would not the difference of velocity between her and the sea she was placed in appear plainly by a violent motion of the ship through the water eastward?

I have not time to explain myself farther, the post waiting; but I believe I have said enough for you to comprehend my meaning. If the reasons hinted at should incline you to think there is any thing in this notion, I should be glad of an answer to this question, if it be capable of a precise answer, viz.

Suppose a ship sails in a northeast line from latitude 39 to latitude 52, in thirty days, how long will she be returning on the same line, winds, currents, &c. being equal? Just so much as the eastern motion of the earth helps her easting, I suppose it will hinder her westing. Perhaps the weight and dimensions or shape of the vessel should be taken into consideration, as the water resists bodies of different shapes differently.

I must beg you to excuse the incorrectness of this scrawl, as I have not time to transcribe. I am, Sir,

Your most humble servant,

B. FRANKLIN.

)M CADWALLADER COLDEN TO B. FRANKLIN.

Answer to the foregoing Letter respecting the Sailing of Ships across the Atlantic.

[Date uncertain.]

SIR,

There is no question but in the case you mention of a ship's being taken up in a southern latitude, and set down in one some degrees more northerly, at the same moment, she would have a degree of motion eastward; but, that it would shorten a voyage from America to Europe, I cannot think; because, as the alteration is made by insensibly small steps, it can only be so much as an alteration of the velocity in the least conceivable part of a degree of latitude is greater than the resistance of the water, which in all cases remains the same, and equally resists the smallest alteration of the velocity, as the greatest.

Suppose, for example, in the alteration of one second of latitude, how much greater will the velocity be in the southward than northward? If it be sixty miles at the equator, what will it be at one second on either side of the equator? The difference is the force which the ship can acquire from the diurnal rotation of the earth in this second. Now it may be asked, whether this difference will be sufficient to overcome the resistance of the water in any degree whatsoever; that is, whether it be not infinitely small in respect to the resistance of the body of water, which resists the motion of a ship; and if so, it can neither add to, nor diminish, the ship's way in the time she alters her latitude one second; and, if this be the case, it cannot either forward or stop her way in a greater change of latitude, because it is done by a continual addition of seconds,

or rather of less than thirds, or of any imaginable quantity; so that, according to my way of computation, in answer to your question, the difference of time in going and return will be = 0.

The shorter voyages to Europe, without doubt, are chiefly owing to the more frequent westerly winds; but this does not account for the fact, that in going to Europe a ship is generally ahead of the reckoning, or you meet with the land sooner than by the computation of the ship's way, if it be well kept; whereas, in coming to America, the reckoning is generally ahead of the ship. The true reason, I think, is from the tides. The high-water every day is nearly three quarters of an hour more easterly on the globe, than the day before; or, following the course of the moon, advancing daily about twelve degrees eastward. And therefore they every day in sailing westerly meet with the contrary current of the water sooner than they do in sailing easterly; as, in a river, in going down you meet the flood sooner than you do the ebb in going up, where the flood and ebb are nearly equal. For the difference of the force and length of the ebb more than the flood in rivers, from the force of the accumulated fresh water, cannot take place at sea.

If you think proper to give this, in your *Miscellany*, as the reason of the shorter voyages from America to Europe than from Europe to America, I will undertake to support it against any objection. There are some mistakes that I would be so far from being ashamed of, after I knew them to be such, that I would be vain of them, because none but those of a lively, quick, and piercing imagination can fall into them. A blockhead is incapable of making such mistakes. There is even pleasure in telling it after the mistake is discovered, and the person to whom it is told values the ingenuity

which occasioned the mistake, and, on reflection, is conscious to himself, that he would have valued himself for that very thought. There is a use likewise in mentioning these mistakes to others, to guard them against those conceptions, that please and flatter the imagination most.

Suppose the difference between the Land's End and the coast of America to be seventy-two degrees of longitude, (I choose this number to avoid fractions.) Then the same high-water on the coast of America will happen six hours later at the Land's End, and therefore a ship will meet with one tide flood against her, which would be an ebb in her favor on the coast of America; and, supposing her voyage performed in thirty days, every day equally approaching to America, she would every day have that tide lessened one thirtieth of the time, as she approaches. Some have had thoughts of calculating an equation to rectify a ship's easting and westing; but, upon reflecting that a general equation cannot serve for every ship, but must be different according to the several moulds by which ships are built, and their being loaded, or in ballast, I believe it will be of little use; for, according to the different moulds of a ship, and her being deep or light, tides have greater or less force on her way. The heavy, dull ships must make much larger allowances than the best sailers, and accordingly we generally find these heavy sailers most out of their reckoning.

I am, &c.

CADWALLADER COLDEN.

TO JARED ELIOT.*

Linseed Oil. — Northeast Storms. — Origin of Springs in Mountains. — Petrified Shells in the Appalachian Mountains. — Observations on a Tariff Law.

Philadelphia, 16 July, 1747.

DEAR SIR,

I received your favor of the 4th instant. I ought before this time to have acknowledged the receipt of the book, which came very safe, and in good order, to hand. We have many oil-mills in this province, it being a great country for flax. Linseed oil may now be bought for three shillings per gallon; sometimes for two shillings and six pence; but at New York, I have been told, it generally holds up at about eight shillings. Of this you can easily be satisfied, it being your neighbour government.

In your last, you inquired about the kind of land from which our hemp is raised. I am told it must be very rich land. Sometimes they use drained swamps and banked meadows; but the greater part of our hemp is brought from Conestago, which is a large and very rich tract of land on the banks of the Susquehanna, a large fresh-water river. It is brought down in wagons.

If you should send any of your steel saws here for sale, I should not be wanting where my recommendation might be of service.

We have had as wet a summer as has been known here these thirty years, so that it was with difficulty

* The Reverend Jared Eliot was a graduate of Yale College, and settled for many years as a clergyman at Killingworth in Connecticut. He had a particular fondness for philosophical studies, and published essays on agriculture, which were much read at the time, and passed through several editions. — EDITOR.

our people got in their harvest. In some parts of the country a great deal of hay has been lost, and some corn mildewed; but in general the harvest has been very great. The two preceding summers (particularly the last) were excessively dry. I think with you, it might be of advantage to know what the seasons are in the several parts of the country. One's curiosity in some philosophical points might also be gratified by it.

We have frequently, along this North American coast, storms from the northeast, which blow violently sometimes three or four days. Of these I have had a very singular opinion some years, viz. that, though the course of the wind is from northeast to southwest, yet the course of the storm is from southwest to northeast; that is, the air is in violent motion in Virginia before it moves in Connecticut, and in Connecticut before it moves at Cape Sable, &c. My reasons for this opinion, (if the like have not occurred to you,) I will give in my next.

I thank you for the curious facts you have communicated to me relating to springs. I think with you, that most springs arise from rains, dews, or ponds, on higher grounds; yet possibly some, that break out near the tops of high hollow mountains, may proceed from the abyss, or from water in the caverns of the earth, rarefied by its internal heat, and raised in vapor, till the cold region near the tops of such mountains condenses the vapor into water again, which comes forth in springs, and runs down on the outside of the mountains, as it ascended on the inside. There is said to be a large spring near the top of Teneriffe; and that mountain was formerly a volcano, consequently hollow within. Such springs, if such there be, may properly be called springs of *distilled* water.

Now I mention mountains, it occurs to tell you, that

the great Appalachian Mountains, which run from York River, back of these colonies, to the Bay of Mexico, show in many places, near the highest parts of them, strata of sea shells; in some places the marks of them are in the solid rocks. It is certainly the *wreck* of a world we live on! We have specimens of these sea-shell rocks, broken off near the tops of these mountains, brought and deposited in our library as curiosities. If you have not seen the like, I will send you a piece. Farther, about mountains (for ideas will string themselves like ropes of onions); when I was once riding in your country, Mr. Walker showed me at a distance the bluff side or end of a mountain, which appeared striped from top to bottom, and told me the stone or rock of that mountain was divided by nature into pillars; of this I should be glad to have a particular account from you. I think I was somewhere near New Haven when I saw it.

You made some mistake when you intended to favor me with some of the new valuable grass seed (I think you called it herd-seed), for what you gave me is grown up, and proves mere timothy; so I suppose you took it out of a wrong paper or parcel.

I wish your new law may have the good effect expected from it, in extricating your government from the heavy debt this war has obliged them to contract. I am too little acquainted with your particular circumstances to judge of the prudence of such a law for your colony with any degree of exactness. But to a friend one may hazard one's notions, right or wrong. And, as you are pleased to desire my thoughts, you shall have them and welcome. I wish they were better.

First, I imagine that the five per cent duty on goods imported from your neighbouring governments, though paid at first hand by the importer, will not upon the

whole come out of his pocket, but be paid in fact by the consumer; for the importer will be sure to sell his goods as much dearer to reimburse himself; so that it is only another mode of taxing your own people, though perhaps meant to raise money on your neighbours. Yet, if you can make some of the goods, heretofore imported, among yourselves, the advanced price of five per cent may encourage your own manufacture, and in time make the importation of such articles unnecessary, which will be an advantage.

Secondly, I imagine the law will be difficult to execute, and require many officers to prevent smuggling in so extended a coast as yours; and the charge considerable; and, if smuggling is not prevented, the fair trader will be undersold and ruined. If the officers are many and busy, there will arise numbers of vexatious lawsuits, and dissensions among your people. *Quære*, whether the advantages will overbalance.

Thirdly, if there is any part of your produce that you can well spare, and would desire to have taken off by your neighbours in exchange for something you more want, perhaps they, taking offence at your selfish law, may in return lay such heavy duties or discouragements on that article, as to leave it a drug on your hands. As to the duty on transporting lumber (unless in Connecticut bottoms to the West Indies), I suppose the design is to raise the price of such lumber on your neighbours, and throw that advanced price into your treasury. But may not your neighbours supply themselves elsewhere? Or, if numbers of your people have lumber to dispose of, and want goods from, or have debts to pay to, your neighbours, will they not (unless you employ numbers of officers to watch all your creeks and landings) run their lumber, and so defeat the law? Or, if the law is strictly executed, and the duty

discourage the transportation to your neighbours, will not all your people, that want to dispose of lumber, be laid at the mercy of those few merchants that send it to the West Indies, who will buy it at their own price, and make such pay for it as they think proper?

If I had seen the law, and heard the reasons that are given for making it, I might have judged and talked of it more to the purpose. At present I shoot my bolt pretty much in the dark; but you can excuse and make proper allowance.

My best respects to good Mrs. Eliot and your sons; and, if it falls in your way, my service to the kind, hospitable people near the River, whose name I am sorry I have forgot.

I am, dear Sir, with the utmost regard,

Your obliged humble servant,

B. FRANKLIN.

TO JARED ELIOT.*

Experiments in the Culture of Grass in Meadows.

SIR,

I have perused your two Essays on Field Husbandry, and think the public may be much benefited by them; but, if the farmers in your neighbourhood are as unwilling to leave the beaten road of their ancestors as they are near me, it will be difficult to persuade them to attempt any improvement. Where the cash is to be laid out on a probability of a return, they are very averse to the running any risk at all, or even expending freely,

* The date of this letter is uncertain; but it was probably written as early as 1747.—EDITOR.

where a gentleman of a more public spirit has given them ocular demonstration of the success.

About eighteen months ago, I made a purchase of about three hundred acres of land near Burlington, and resolved to improve it in the best and speediest manner, that I might be enabled to indulge myself in that kind of life, which was most agreeable. My fortune, thank God, is such that I can enjoy all the necessaries and many of the indulgences of life; but I think that in duty to my children I ought so to manage, that the profits of my farm may balance the loss my income will suffer by my retreat to it. In order to this, I began with a meadow, on which there had never been much timber, but it was always overflowed. The soil is very fine, and black about three feet; then it comes to a bluish clay. Of this deep meadow I have about eighty acres, forty of which had been ditched and mowed. The grass which comes in first after ditching is spear-grass and white clover; but the weeds are to be mowed four or five years before they will be subdued, as the vegetation is very luxuriant.

This meadow had been ditched and planted with Indian corn, of which it produced above sixty bushels per acre. I first scoured up my ditches and drains, and took off all the weeds; then I ploughed it, and sowed it with oats in the last of May. In July I mowed them down, together with the weeds, which grew plentifully among them, and they made good fodder. I immediately ploughed it again, and kept harrowing till there was an appearance of rain; and, on the 23d of August, I sowed near thirty acres with red clover and herd-grass, allowing six quarts of herd-grass and four pounds of red clover to an acre in most parts of it; in other parts, four quarts of herd-grass and three pounds of red clover. The red clover came up in four days, and

the herd-grass in six days; and I now find, that, where I allowed the most seed, it protects itself the best against the frost. I also sowed an acre with twelve pounds of red clover, and it does well. I sowed an acre more with two bushels of rye-grass seed and five pounds of red clover; the rye-grass seed failed, and the red clover heaves out much for want of being thicker. However, in March next I intend to throw in six pounds more of red clover, as the ground is open and loose. As these grasses are represented not durable, I have sown two bushels of the sweeping of hay-lofts (where the best hay was used), well riddled, per acre, supposing that the spear-grass and white clover seed would be more equally scattered when the other shall fail.

What surprised me was to find, that the herd-grass, whose roots are small and spread near the surface, should be less affected by the frost than the red clover, whose roots I measured in the last of October, and found that many of their tap roots penetrated five inches, and from its sides threw out near thirty horizontal roots, some of which were six inches long, and branched. From the figure of this root, I flattered myself, that it would endure the heaving of the frost; but I now see, that wherever it is thin sown it is generally hove so far out, that but a few of the horizontal and a small part of the tap roots remain covered, and I fear will not recover. Take the whole together, it is well matted, and looks like a green corn-field.

I have about ten acres more of this ground ready for seed in the spring, but expect to combat with the weeds a year or two. That sown in August I believe will rise so soon in the spring, as to suppress them in a great measure.

My next undertaking was a round pond of twelve acres. Ditching round it, with a large drain through the

middle, and other smaller drains, laid it perfectly dry. This, having first taken up all the rubbish, I ploughed up, and harrowed it many times over, till it was smooth. Its soil is blackish; but, in about a foot or ten inches, you come to a sand of the same color with the upland. From the birch that grew upon it, I took it to be of a cold nature, and therefore I procured a grass which would best suit that kind of ground, intermixed with many others, that I might thereby see which suited it best. On the 8th of September, I laid it down with rye, which being harrowed in, I threw in the following grass seed; a bushel of Salem grass or feather-grass, half a bushel of timothy or herd-grass, half a bushel of rye-grass, a peck of burden-grass or blue bent, and two pints of red clover per acre, (all the seed in the chaff, except the clover,) and bushed them in. I could wish they had been clean, as they would have come up sooner, and been better grown before the frost; and I have found by experiment, that a bushel of clean chaff of timothy or Salem grass will yield five quarts of seed. The rye looks well, and there is abundance of timothy or Salem grass come up amongst it; but it is yet small, and in that state there is scarce any knowing those grasses apart. I expect from the sand lying so near the surface, that it will suffer much in dry weather.

B. FRANKLIN.

TO THOMAS HOPKINSON.*

On the Vis Inertiæ of Matter.

Philadelphia, 1747.

ACCORDING to my promise, I send you *in writing* my observations on your book;† you will be the better able to consider them; which I desire you to do at your leisure, and to set me right where I am wrong.

I stumble at the threshold of the building, and therefore have not read farther. The author's *vis*

* Thomas Hopkinson was born in London, in April, 1709. [illegible] sessed a fine genius, and a finished education, having been a [illegible] Oxford. He came to America while young, and married and settled in Philadelphia, where he died in 1751. He was distinguished for his classical attainments, general learning, the brilliancy of his conversation, and his fondness for philosophical studies. Being an intimate friend of Franklin, he was associated with him in his electrical and philosophical experiments. In a note to one of his letters on electricity, Franklin says; "The power of points to *throw off* the electrical fire was first communicated to me by my ingenious friend, Mr. Thomas Hopkinson, since deceased, whose virtue and integrity in every station of his life, public and private, will ever make his memory dear to those who knew him, and knew how to value him." He mentions other assistance derived from the observations and experiments of his friend. It is an honorable proof of Mr. Hopkinson's talents, learning, and character, that he was chosen the first president of the *American Philosophical Society*, instituted in the year 1744. (See above, p. 29.) He also took an active part in founding the City Library and the College of Philadelphia, and was a zealous promoter of public institutions and improvements. He left several children, among whom was Francis Hopkinson, one of the signers of the Declaration of Independence, well known as a writer of genius, wit, and ability, and for his valuable public services during the revolution and subsequently to that event. — EDITOR.

† It was a book by Andrew Baxter, entitled *An Inquiry into the Nature of the Human Soul, wherein its Immateriality is evinced*, &c. One of the chief objects of this book was to prove, that a resistance to any change is essential to matter, consequently inconsistent with *active* powers in it; and that, if matter wants active powers, an *immaterial being* is necessary for all those effects, &c. ascribed to its own natural powers. After stating the several proofs, questioned by Dr. Franklin, of a VIS

essential to matter, upon which the whole work is founded, I have not been able to comprehend. And I do not think he demonstrates at all clearly (at least to me he does not), that there is really *such a property in matter.*

He says, No. 2, "Let a given body or mass of matter be called *a*, and let any given celerity be called *c*. That *celerity* doubled, tripled, &c., or halved, thirded, &c., will be $2c$, $3c$, &c., or $\frac{1}{2}c$, $\frac{1}{3}c$, &c., respectively. Also the *body* doubled, trepled, or halved, thirded, will be $2a$, $3a$, or $\frac{1}{2}a$, $\frac{1}{3}a$, respectively." Thus far is clear. But he adds, "Now to move the body *a*, with the celerity *c*, requires a certain force to be impressed upon it; and to move it with a celerity as $2c$, requires *twice that force* to be impressed upon it, &c." Here I suspect some mistake creeps in, by the author's not distinguishing between a great force applied at once, and a small one continually applied, to a mass of matter, in order to move it. I think it is generally allowed by the philosophers, and, for aught we know, is certainly true, that there is no mass of matter, how great soever, but may be moved by any force how small soever, (taking friction out of the question,) and this small force, continued, will in time bring the mass to move with any velocity whatsoever. Our author himself seems to allow this

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inertiæ, or "*force* of inertness," in matter, the author adds; "If the immateriality of the soul, the existence of God, and the necessity of a most particular, incessant providence in the world, are demonstrable from such plain and easy *principles*, the atheist has a desperate cause in hand." (See the third edition, pp. 1–8.) In fact, Mr. Baxter's doctrine seems to establish, rather than disprove, an activity in matter, and consequently to defeat his own conclusion, were not that conclusion to be found from other premises. *Primâ facie*, it seems better for Mr. Baxter's system to suppose matter *incapable* of *force* or effort, even in the case, as he calls it, of resisting change; which case appears to me no other than the simple one, of matter *not* altering its state *without* a cause, and a cause exactly proportioned to the effect. — B. V.

towards the end of the same No. 2, when he is subdividing his celerities and forces; for as in continuing the division to eternity by his method of $\frac{1}{2}c$, $\frac{1}{3}c$, $\frac{1}{4}c$, &c. you can never come to a fraction of velocity that is equal to $0c$, or no celerity at all; so, dividing the force in the same manner, you can never come to a fraction of force that will not produce an equal fraction of celerity.

Where then is the mighty *vis inertiæ*, and what is its strength, when the greatest assignable mass of matter will give way to, or be moved by, the *least* assignable force? Suppose two globes equal to the sun and to one another, exactly equipoised in Jove's balance; suppose no friction in the centre of motion, in the beam or elsewhere; if a musqueto then were to light on one of them, would he not give motion to them both, causing one to descend and the other to rise? If it is objected, that the force of gravity helps one globe to descend, I answer, the same force opposes the other's rising. Here is an equality that leaves the whole motion to be produced by the musqueto, without whom those globes would not be moved at all. What then does *vis inertiæ* do in this case? and what other effect could we expect *if there were no such thing?* Surely, if it were any thing more than a phantom, there might be enough of it in such *vast* bodies to annihilate, by its opposition to motion, so trifling a force!

Our author would have reasoned more clearly, I think, if, as he has used the letter a for a certain quantity of matter, and c for a certain quantity of celerity, he had employed one letter more, and put f, perhaps, for a certain quantity of force. This let us suppose to be done; and then, as it is a maxim that the force of bodies in motion is equal to the quantity of matter multiplied by the celerity, (or $f = c \times a$); and as the force

received by and subsisting in matter, when it is put in motion, can never exceed the force given; so, if f moves a with c, there must needs be required $2f$ to move a with $2c$; for a moving with $2c$ would have a force equal to $2f$, which it could not receive from $1f$; and this, not because there is such a thing as *vis inertiæ*, for the case would be the same *if that had no existence;* but because nothing can give more than it has. And now again, if a thing *can* give what it has, if $1f$ can to $1a$ give $1c$, which is the same thing as giving it $1f$, (that is, if force applied to matter at rest, can put it in motion, and give it *equal* force,) where then is *vis inertiæ?* If it existed at all in matter, should we not find the quantity of its resistance subtracted from the force given?

In No. 4, our author goes on and says, "The body a requires a certain force to be impressed on it to be moved with a celerity as c, or such a force is necessary; and therefore it makes a certain resistance, &c.; a body as $2a$ requires *twice* that force to be moved with the *same celerity*, or it makes twice that resistance; and so on." This I think is not true; but that the body $2a$, moved by the force $1f$, (though the eye may judge otherwise of it) does really move with the same celerity as it did when impelled by the same force; for $2a$ is compounded of $1a + 1a$; and if each of the $1a$'s, or each part of the compound, were made to move with $1c$ (as they might be by $2f$), then the whole would move with $2c$, and not with $1c$, as our author supposes. But $1f$ applied to $2a$ makes each a move with $\frac{1}{2}c$; and so the whole moves with $1c$; exactly the same as $1a$ was made to do by $1f$ before. What is equal celerity but a *measuring the same space by moving bodies in the same time?* Now if $1a$, impelled by $1f$, measures one hundred yards in a minute; and in $2a$, impelled by $1f$, each

a measures fifty yards in a minute, which added make one hundred; are not the celerities, as the forces, equal? And, since force and celerity in the same quantity of matter are always in *proportion* to each other, why should we, when the quantity of matter is doubled, allow the force to continue unimpaired, and yet suppose one half of the celerity to be lost? * I wonder the more at our author's mistake in this point, since in the same number I find him observing; "We may easily conceive that a body, as $3a$, $4a$, &c., would make three or four bodies equal to once a, each of which would require once the first force to be moved with the celerity c." If then, in $3a$, each a requires once the first force f, to be moved with the celerity c, would not each move with the force f, and celerity c? and consequently the whole be $3a$ moving with $3f$ and $3c$? After so distinct an observation, how could he miss of the consequence, and imagine that $1c$ and $3c$ were the same? Thus, as our author's abatement of celerity in

* Dr. Franklin's reasoning seems only to prove, that, where bodies of different masses have equal force, they "measure *equal* space in equal times." For, allowing that $2a$ moves one hundred yards in a minute (because it moves two separate fifty yards in that time), yet surely that space is not the *same* with that of the one hundred yards moved by $1a$, in the same time, though it may be equal to it; for the body $2a$ (that is, a and a), in the first case, describes a broad double space; and the body $1a$, in the second case, describes a long and single space. There is a farther consideration which may show the difference of celerity and force. For when Dr. Franklin says, in his second paragraph, that "there is no mass of matter, how great soever, but may be moved, *with any velocity*, by any continued force, how small soever," I ask whether the *moving body* must not have its force rather in the shape of much celerity, than of much matter, for this purpose; since without much celerity it would not move fast enough to *apply* its force to give the required velocity; even though its quantity of matter, and consequently of force, were infinite. "Equal celerity therefore in moving bodies is their measuring equal space, *along a continued line*, in equal time." Equal space measured along a number of *smaller parallel* lines, suits cases of *equal motion* indeed, but, according to this corrected definition, not of *equal celerity*. — B. V

the case of 2*a* moved by 1*f* is imaginary, so must be his additional resistance. And here again, I am at a loss to discover any effect of the *vis inertiæ*.

In No. 6, he tells us, "that all this is likewise certain when taken the contrary way, viz. *from motion to rest;* for the body *a*, moving with a certain velocity, as *c*, requires a certain degree of force or resistance to stop that motion," &c. &c.; that is, in other words, equal force is necessary to destroy force. It may be so. But how does that discover a *vis inertiæ?* Would not the effect be the same, *if there were no such thing?* A force 1*f* strikes a body 1*a*, and moves it with the celerity 1*c*, that is, with the force 1*f*; it requires, even according to our author, only an opposing 1*f* to stop it. But ought it not (if there were a *vis inertiæ*) to have not only the force 1*f*, but an additional force equal to the force of *vis inertiæ*, that *obstinate power by which a body endeavours with* all its might *to continue in its present state, whether of motion or rest?* I say, ought there not to be an opposing force equal to the sum of these? The truth, however, is, that there is no body, how large soever, moving with any velocity, how great soever, but may be stopped by any opposing force, how small soever, continually applied. At least, all our modern philosophers agree to tell us so.

Let me turn the thing in what light I please, I cannot discover the *vis inertiæ*, nor any effect of it. It is allowed by all, that a body 1*a*, moving with a velocity 1*c*, and a force 1*f*, *striking another* body 1*a* at rest, they will afterwards *move on together*, each with $\frac{1}{2}c$ and $\frac{1}{2}f$; which, as I said before, is equal in the whole to 1*c* and 1*f*. If *vis inertiæ*, as in this case, neither abates the force nor the velocity of bodies, what does it, or how does it discover itself?

I imagine I may venture to conclude my observations

on this piece, almost in the words of the author; that, if the doctrines of the immateriality of the soul and the existence of God, and of divine providence, are demonstrable from no plainer principles, the *deist* (that is, *theist*) has a desperate cause in hand. I oppose *my theist* to his atheist, because I think they are diametrically opposite; and not near of kin, as Mr. Whitefield seems to suppose, where (in his Journal) he tells us, "*M. B. was a deist, I had almost said an atheist;*" that is, *chalk*, I had almost said *charcoal.*

The din of the Market* increases upon me; and that, with frequent interruptions, has, I find, made me say some things twice over; and, I suppose, forget some others I intended to say. It has, however, one good effect, as it obliges me to come to the relief of your patience with

Your humble servant,

B. FRANKLIN.

TO CADWALLADER COLDEN.

Baxter's Book on the Vis Inertiæ of Matter. — Manufacture of Electrical Apparatus. — Colden's Philosophical Treatise.

Philadelphia, 6 August, 1747.

SIR,

The observations I sent you on Baxter's book were wrote on a sheet or two of paper in folio.† He builds his whole argument on the *vis inertiæ* of matter. I

* Philadelphia Market, near which Dr. Franklin lived.

† Probably the same in substance as the preceding letter to Mr. Hopkinson. — EDITOR.

boldly denied the being of such a property, and endeavoured to demonstrate the contrary. If I succeeded, all his edifice falls of course, unless some other way supported. I desired your sentiments of my argument. You left the book for me at New York, with a few lines containing a short censure upon the author, and that your time had been much taken up in town with business, but you were now about to retire into the country, where you should have leisure to peruse my papers; since which I have heard nothing from you relating to them. I hope you will easily find them, because I have lost my rough draft; but do not give yourself much trouble about them; for if they are lost, it is really no great matter.

I am glad to hear, that some gentlemen with you are inclined to go on with electrical experiments. I am satisfied we have workmen here, who can make the apparatus as well to the full as that from London; and they will do it reasonably. By the next post, I will send you their computation of the expense. If you shall conclude to have it done here, I will oversee the work, and take care that every part be done to perfection, as far as the nature of the thing admits.

Instead of the remainder of my rough minutes on electricity, (which are indeed too rough for your view,) I send you enclosed copies of two letters I lately wrote to Mr. Collinson on that subject. When you have perused them, please to leave them with Mr. Nichols, whom I shall desire to forward them per next post to a friend in Connecticut.

I am glad your Philosophical Treatise meets with so good reception in England. Mr. Collinson writes the same things to Mr. Logan; and Mr. Rose, of Virginia, writes me, that he had received accounts from his correspondents to the same purpose. I perceive by the

papers, that they have also lately reprinted, in London, your "History of the Five Nations" in octavo. If it come to your hands, I should be glad to have a sight of it.

Mr. Logan,* on a second reading of your piece on Fluxions lately, is satisfied, that some of the faults he formerly objected to it were his own, and owing to his too little attention at that time. He desires me to tell you so, and that he asks your pardon. Upon what Mr. Collinson wrote, he again undertook to read and consider your Philosophical Treatise.† I have not seen

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* James Logan, who came to America with William Penn, and who was distinguished for his attainments in classical literature, and in almost every branch of science, as well as for his public services for nearly half a century in Pennsylvania.—EDITOR.

† The title of this treatise, as originally printed, was as follows; "*Explication of the first Causes of Action in Matter; and of the Cause of Gravitation. London*, 1746." A second edition enlarged was published five years afterwards with a different title, namely; "*The Principles of Action in Matter, the Gravitation of Bodies and the Motion of the Planets explained from those Principles. By Cadwallader Colden, Esquire. London. Printed for Dodsley.* 1751." The book was dedicated to the Earl of Macclesfield, then President of the Royal Society. Appended is a chapter entitled, "An Introduction to the Doctrine of Fluxions, or the Arithmetic of Infinities; in order to assist the Imagination in forming Conceptions of the Principles on which that Doctrine is founded." The volume contains eight chapters, besides the one on Fluxions, is printed in quarto, and extends to two hundred and fifteen pages.

Mr. Colden seems not to have been satisfied with the manner in which the first edition found its way to the public. In the Preface to the second edition he says;

"A few copies of the two first chapters of this treatise were published at New York, with design to know the sentiments of the learned on new principles in natural philosophy or physics, which were advanced in that essay. The London edition was without the author's knowledge, and had never been made, could he have prevented it. Since that time, he has been encouraged to go on. In this edition the two first chapters are revised, the matters contained in them follow more consequentially, some obscurities are removed, the sentiments, it is hoped, put in a clearer light, and some new theorems added. In the following chapters, which were not before published, these principles are applied to the explication

him since, but shall soon, and will send you his sentiments. I am, Sir,

With great respect,

Your most humble servant,

B. FRANKLIN.

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of the motions of the planets, and of the phenomena thereon depending, and of some other general phenomena, the causes of which, the author thinks, have not been before discovered."

The following extract from the Preface will exhibit to the reader an outline of Mr. Colden's views, and of the principles he attempts to establish.

"It is laid down as a principle, that all the primary or simple ideas we have of things external to us arise from the impressions or actions of these things on our senses; and, therefore, that the properties and qualities of things are nothing else but their various actions, or modes of acting, either simply or complicated; that the knowledge we have of things is no more than the perception of these actions, of their different degrees and different modes, and of the ratios of these differences to each other.

"That all simple beings or things have one single action, or manner of acting, essential to them; without which we have no conception of a thing.

"That there are two, and perhaps only two, essential different modes of action in material beings. The one, a power by which the thing, in which this action subsists, does resist all change of its present state; the other, a power by which the thing, in which the action of moving subsists, is continually changing its present state, or situation, by motion, and gives motion to every other thing, which at any time moves.

"It is a self-evident proposition, that nothing acts where it is not; therefore, if any thing exert any action at a distance, this action must be communicated to that distance, by some medium from the place of the acting thing, to the place where the action is communicated. The mutual apparent attraction of bodies at a distance from each other shows the necessity of the existence of such a medium. This medium makes a third kind of matter, essentially different from the other two, by its equally receiving the action, or manner of acting, either of the resisting or of the moving power, and by its reacting those actions with the same degree of force or action it received them. From the nature of this medium (commonly called ether), or from the necessary consequences of receiving and reacting these contrary modes of action, the apparent mutual attraction of bodies at a distance from each other, and gravitation, are explained, and the several phenomena thence arising.

"Every thing to which any action is essential, must exert that action equally in all directions; because nothing can be conceived in the thing

*A Conjecture as to the Cause of the Heat of the Blood in Health, and of the Cold and Hot Fits of some Fevers.**

The parts of fluids are so smooth, and roll among one another with so little friction, that they will not by any (mechanical) agitation grow warmer. A phial half full of water shook with violence and long continued, the water neither heats itself nor warms the phial. Therefore the blood does not acquire its heat either from the motion and friction of its own parts, or its friction against the sides of its vessels.

But the parts of solids, by reason of their closer adhesion, cannot move among themselves without friction, and that produces heat. Thus, bend a plummet to and fro, and, in the place of bending, it shall soon grow hot. Friction on any part of our flesh heats it. Clapping of the hands warms them. Exercise warms the whole body.

The heart is a thick muscle, continually contracting and dilating near eighty times in a minute. By this

itself to hinder it, in one direction more than another. Then the direction of motion in the moving power, towards any one point more than towards any other point, must be by something external, by the resistance in that particular direction being less than in any other.

"Several arguments are produced, in this essay, to demonstrate, that *light* is the substance or thing to which the power of moving is essential; and to those therein mentioned, among which the principal is the demonstrating in what manner the motions of the planets and comets arise from thence, this other argument may be added, that we can have no conception of light without motion, of which any one may convince himself by a proper attention. For example, if light be supposed to be composed of small globular bodies at rest, this supposition gives no idea of light or colors; it conveys no idea of any thing in common with the ideas raised in our mind by the action of light." — Editor.

* This piece I have found in Franklin's handwriting among the papers of Cadwallader Colden. Its date is uncertain, but it was probably written before the year 1750. — Editor.

motion there must be a constant interfrication of its constituent solid parts. That friction must produce a heat, and that heat must consequently be continually communicated to the perfluent blood.

To this may be added, that every propulsion of the blood by the contraction of the heart, distends the arteries, which contract again in the intermission; and this distension and contraction of the arteries may occasion heat in them, which they must likewise communicate to the blood that flows through them.

That these causes of the heat of the blood are sufficient to produce the effect, may appear probable, if we consider that a fluid once warm requires no more heat to be applied to it in any part of time to keep it warm, than what it shall lose in an equal part of time. A smaller force will keep a pendulum going, than what first set it in motion.

The blood, thus warmed in the heart, carries warmth with it to the very extremities of the body, and communicates it to them; but, as by this means its heat is gradually diminished, it is returned again to the heart by the veins for a fresh calefaction.

The blood communicates its heat, not only to the solids of our body, but to our clothes, and to a portion of the circumambient air. Every breath, though drawn in cold, is expired warm; and every particle of the *materia perspirabilis* carries off with it a portion of heat.

While the blood retains a due fluidity, it passes freely through the minutest vessels, and communicates a proper warmth to the extremities of the body. But when by any means it becomes so viscid, as not to be capable of passing those minute vessels, the extremities, as the blood can bring no more heat to them, grow cold.

The same viscidity in the blood and juices checks

or stops the perspiration, by clogging the perspiratory ducts, or, perhaps, by not admitting the perspirable parts to separate. Paper wet with size and water will not dry so soon as if wet with water only.

A vessel of hot water, if the vapor can freely pass from it, soon cools. If there be just fire enough under it to add continually the heat it loses, it retains the same degree. If the vessel be closed, so that the vapor may be retained, there will from the same fire be a continual accession of heat to the water, till it rises to a great degree. Or, if no fire be under it, it will retain the heat it first had for a long time. I have experienced, that a bottle of hot water stopped, and put in my bed at night, has retained so much heat seven or eight hours, that I could not in the morning bear my foot against it, without some of the bedclothes intervening.

During the cold fit, then, perspiration being stopped, great part of the heat of the blood, that used to be dissipated, is confined and retained in the body; the heart continues its motion, and creates a constant accession to that heat; the inward parts grow very hot, and, by contact with the extremities, communicate that heat to them. The glue of the blood is by this heat dissolved, and the blood afterwards flows freely, as before the disorder.

TO PETER COLLINSON.

*Magical Square of Squares.**

SIR,

According to your request, I now send you the arithmetical curiosity, of which this is the history.

Being one day in the country, at the house of our common friend, the late learned Mr. Logan, he showed me a folio French book filled with magic squares, wrote, if I forget not, by one M. Frenicle, in which, he said, the author had discovered great ingenuity and dexterity in the management of numbers; and, though several other foreigners had distinguished themselves in the same way, he did not recollect that any one Englishman had done any thing of the kind remarkable.

I said, it was, perhaps, a mark of the good sense of our English mathematicians, that they would not spend their time in things that were merely *difficiles nugæ*, incapable of any useful application. He answered, that many of the arithmetical or mathematical questions, publicly proposed and answered in England, were

* The dates of the letters, in which the account of Magical Squares and Magical Circles was communicated to Mr. Collinson, are not known; but in a letter from James Logan to Mr. Collinson, dated February 14th, 1750, the following mention is made of them. "Our Benjamin Franklin," says Mr. Logan, "is certainly an extraordinary man, one of a singular good judgment, but of equal modesty. He is clerk of our Assembly, and there, for want of other employment, while he sat idle, he took it into his head to think of *magical squares*, in which he outdid Frenicle himself, who published above eighty pages in folio on that subject alone."

In reply to a letter from Mr. Logan on this subject, Franklin wrote (January 20th, 1749–50,) "The magical squares, how wonderful soever they may seem, are what I cannot value myself upon, but am rather ashamed to have it known I have spent any part of my time in an employment that cannot possibly be of any use to myself or others."—EDITOR.

equally trifling and useless. "Perhaps the considering and answering such questions," I replied, "may not be altogether useless, if it produces by practice an habitual readiness and exactness in mathematical disquisitions, which readiness may, on many occasions, be of real use." "In the same way," says he, "may the making of these squares be of use." I then confessed to him, that in my younger days, having once some leisure, (which I still think I might have employed more usefully,) I had amused myself in making this kind of magic squares, and, at length, had acquired such a knack at it, that I could fill the cells of any magic square of reasonable size, with a series of numbers, as fast as I could write them, disposed in such a manner as that the sums of every row, horizontal, perpendicular, or diagonal, should be equal; but not being satisfied with these, which I looked on as common and easy things, I had imposed on myself more difficult tasks, and succeeded in making other magic squares, with a variety of properties, and much more curious. He then showed me several in the same book, of an uncommon and more curious kind; but, as I thought none of them equal to some I remembered to have made, he desired me to let him see them; and accordingly, the next time I visited him, I carried him a square of eight, which I found among my old papers, and which I will now give you, with an account of its properties. (See Plate VII. Fig. 1.)

The properties are,

1. That every straight row (horizontal or vertical) of eight numbers added together makes 260, and half each row half 260.

2. That the bent row of eight numbers, ascending and descending diagonally, viz. from 16 ascending to 10, and from 23 descending to 17; and every one

of its parallel bent rows of eight numbers, make 260. Also the bent row from 52 descending to 54, and from 43 ascending to 45, and every one of its parallel bent rows of eight numbers, make 260. Also the bent row from 45 to 43, descending to the left, and from 23 to 17, descending to the right, and every one of its parallel bent rows of eight numbers, make 260. Also the bent row from 52 to 54, descending to the right, and from 10 to 16, descending to the left, and every one of its parallel bent rows of eight numbers, make 260. Also the parallel bent rows next to the abovementioned, which are shortened to three numbers ascending and three descending, &c., as from 53 to 4 ascending, and from 29 to 44 descending, make, with the two corner numbers, 260. Also the two numbers, 14, 61, ascending, and 36, 19, descending, with the lower four numbers situated like them, viz. 50, 1, descending, and 32, 47, ascending, make 260. And, lastly, the four corner numbers, with the four middle numbers, make 260.

So this magical square seems perfect in its kind. But these are not all its properties; there are five other curious ones, which, at some other time, I will explain to you.

Mr. Logan then showed me an old arithmetical book, in quarto, wrote, I think, by one Stifelius, which contained a square of sixteen, that he said he should imagine must have been a work of great labor; but, if I forget not, it had only the common properties of making the same sum, viz. 2056, in every row, horizontal, vertical, and diagonal. Not willing to be outdone by Mr. Stifelius, even in the size of my square, I went home, and made, that evening, the following magical square of sixteen, which, besides having all the properties of the foregoing square of eight, that is, it would make the 2056 in all the same rows and diago-

MAGICAL SQUARES.

Fig. 2.

200	217	232	249	8	25	40	57	72	89	104	121	136	153	168	181
58	39	26	7	250	231	218	199	186	167	154	135	122	103	90	71
198	219	230	251	6	27	38	59	70	91	102	123	134	155	166	187
60	37	28	5	252	229	220	197	188	165	156	133	124	101	92	69
201	216	233	248	9	24	41	56	73	88	105	120	137	152	169	184
55	42	23	10	247	234	215	202	183	170	151	138	119	106	87	74
20	244	235	246	11	22	43	54	75	86	107	118	139	150	171	182
53	14	31	12	245	236	213	204	181	172	149	140	117	108	85	76
205	212	237	244	13	30	45	52	77	84	109	116	141	148	173	180
51	16	29	14	243	238	211	206	179	174	147	142	115	110	83	78
207	210	239	242	15	18	47	50	79	82	111	114	143	146	175	178
49	18	17	16	241	240	209	208	177	176	145	144	113	112	81	80
192	225	224	257	4	29	36	61	68	93	100	125	132	157	164	189
62	35	34	3	254	227	222	195	190	163	158	131	126	99	94	67
194	223	226	255	2	31	34	63	66	95	98	127	130	159	162	191
64	33	32	1	256	225	224	193	192	161	160	129	128	97	96	65

Fig. 1.

32	61	4	13	20	29	36	45
14	3	62	31	46	35	30	19
33	60	5	12	21	28	37	44
11	6	59	34	43	38	27	22
55	58	7	10	23	26	39	42
9	8	57	36	41	40	25	24
50	63	2	15	18	51	34	47
16	1	64	49	48	33	32	17

nals, had this added, that a four-square hole being cut in a piece of paper of such a size as to take in and show through it just sixteen of the little squares, when laid on the greater square, the sum of the sixteen numbers, so appearing through the hole, wherever it was placed on the greater square, should likewise make 2056. This I sent to our friend the next morning, who, after some days, sent it back in a letter with these words; "I return to thee thy astonishing or most stupendous piece of the magical square, in which" — but the compliment is too extravagant, and therefore, for his sake, as well as my own, I ought not to repeat it. Nor is it necessary; for I make no question but you will readily allow this square of sixteen to be the most magically magical of any magic square ever made by any magician. (See Plate VII. Fig. 2.)

I did not, however, end with squares, but composed also a magic circle, consisting of eight concentric circles, and eight radial rows, filled with a series of numbers from 12 to 75 inclusive, so disposed as that the numbers of each circle, or each radial row, being added to the central number 12, they make exactly 360, the number of degrees in a circle; and this circle had, moreover, all the properties of the square of eight. If you desire it, I will send it; but at present, I believe, you have enough on this subject.

I am, &c.

B. FRANKLIN.

TO PETER COLLINSON.

Magical Circle.

SIR,

I am glad the perusal of the magical squares afforded you any amusement. I now send you the magical circle. (See Plate VIII.)

Its properties, besides those mentioned in my former, are these.

Half the numbers in any radial row, added with half the central number, make 180, equal to the number of degrees in a semicircle.

Also half the numbers in any one of the concentric circles, taken either above or below the horizontal double line, with half the central number, make 180.

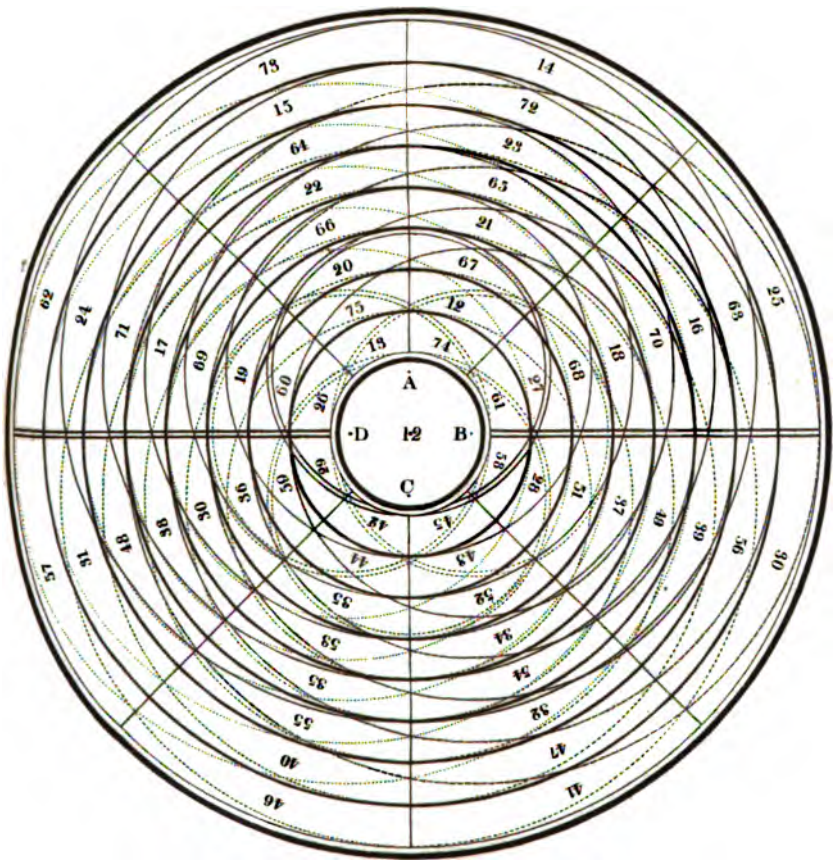
And, if any four adjoining numbers, standing nearly in a square, be taken from any part, and added with half the central number, they make 180.

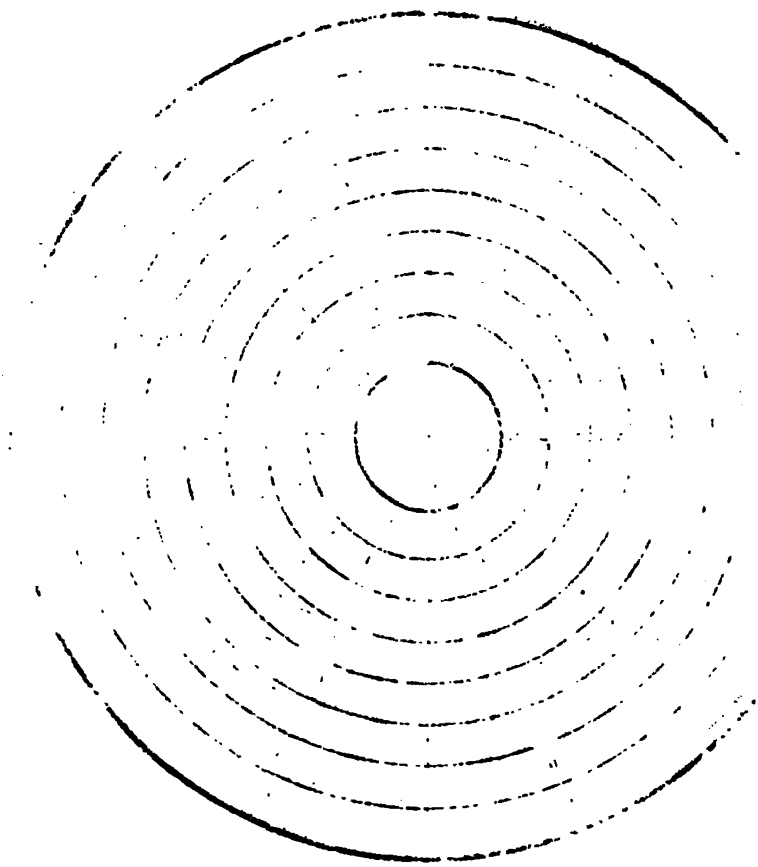
There are, moreover, included four other sets of circular spaces, eccentric with respect to the first, each of these sets containing five spaces. The centres of the circles that bound them, are at *A*, *B*, *C*, and *D*. Each set, for the more easy distinguishing them from the first, are drawn with a different colored ink, red, blue, green, and yellow.*

These sets of eccentric circular spaces intersect those of the concentric, and each other; and yet the numbers contained in each of the twenty eccentric spaces, taken all around, make, with the central number, the same sum as those in each of the eight concentric, viz. 360. The halves, also of those drawn from the centres

* In the plate they are distinguished by dashed or dotted lines, as different as the engraver could well make them.

MAGIC CIRCLE OF CIRCLES.





A and *C*, taken above or below the double horizontal line, and of those drawn from centres, *B* and *D*, taken to the right or left of the vertical line, do, with half the central number, make just 180.

It may be observed, that there is not one of the numbers but what belongs at least to two of the different circular spaces; some to three, some to four, some to five; and yet they are all so placed as never to break the required number 360, in any of the twenty-eight circular spaces within the primitive circle.

These interwoven circles make so perplexed an appearance, that it is not easy for the eye to trace every circle of numbers one would examine, through all the maze of circles intersected by it; but, if you fix one foot of the compasses in either of the centres, and extend the other to any number in the circle you would examine belonging to that centre, the moving foot will point the others out, by passing round over all the numbers of that circle successively. I am, &c.

B. FRANKLIN.

TO JARED ELIOT.

Northeast Storms begin at the South. — Account of a Copper Mine. — Subscriptions for an Academy in Philadelphia.

Philadelphia, 13 February, 1749 - 50.

DEAR SIR,

You desire to know my thoughts about the northst storms beginning to leeward. Some years since, ere was an eclipse of the moon at nine o'clock in the vening, which I intended to observe; but before night storm blew up at northeast, and continued violent all

night and all next day; the sky thick-clouded, dark, and rainy, so that neither moon nor stars could be seen. The storm did a great deal of damage all along the coast, for we had accounts of it in the newspapers from Boston, Newport, New York, Maryland, and Virginia; but what surprised me was, to find in the Boston newspapers an account of an observation of that eclipse made there; for I thought, as the storm came from the northeast, it must have begun sooner at Boston than with us, and consequently have prevented such observation. I wrote to my brother about it, and he informed me, that the eclipse was over there an hour before the storm began. Since which I have made inquiries from time to time of travellers, and of my correspondents northeastward and southwestward, and observed the accounts in the newspapers from New England, New York, Maryland, Virginia, and South Carolina; and I find it to be a constant fact, that northeast storms begin to leeward; and are often more violent there than farther to windward. Thus the last October storm, which with you was on the 8th, begun on the 7th in Virginia and North Carolina, and was most violent there.*

As to the reason of this, I can only give you my conjectures. Suppose a great tract of country, land and sea, to wit, Florida and the Bay of Mexico, to have clear weather for several days, and to be heated by the sun, and its air thereby exceedingly rarefied. Suppose the country northeastward, as Pennsylvania, New Eng-

* Professor Bache, of the University of Pennsylvania, has shown, that the eclipse of the moon, here alluded to, happened in the evening of the 21st of October, 1743; as may be seen in his tract entitled, "An Attempt to fix the Date of the Observation of Dr. Franklin, in Relation to the Northeast Storms of the Atlantic Coast of the United States," published in the *Journal of the Franklin Institute*, in the year 1833. It appears that Dr. Franklin was the first discoverer of the above facts respecting northeast storms. — EDITOR.

land, Nova Scotia, and Newfoundland, to be at the same time covered with clouds, and its air chilled and condensed. The rarefied air being lighter must rise, and the denser air next to it will press into its place; that will be followed by the next denser air, that by the next, and so on. Thus, when I have a fire in my chimney, there is a current of air constantly flowing from the door to the chimney; but the beginning of the motion was at the chimney, where the air being rarefied by the fire rising, its place was supplied by the cooler air that was next to it, and the place of that by the next, and so on to the door. So the water in a long sluice or mill-race, being stopped by a gate, is at rest like the air in a calm; but as soon as you open the gate at one end to let it out, the water next the gate begins first to move, that which is next to it follows; and so, though the water proceeds forward to the gate, the motion which began there runs backwards, if one may so speak, to the upper end of the race, where the water is last in motion. We have on this continent a long ridge of mountains running from northeast to southwest; and the coast runs the same course. These may, perhaps, contribute towards the direction of the winds, or at least influence them in some degree. If these conjectures do not satisfy you, I wish to have yours on the subject.

I doubt not but those mountains, which you mention, contain valuable mines, which time will discover. I know of but one valuable copper mine in this country, which is that of Schuyler's in the Jerseys. This yields good copper, and has turned out vast wealth to the owners. I was at it last fall, but they were not then at work. The water is grown too hard for them, and they waited for a fire-engine from England to drain their pits. I suppose they will have that at work next summer; it costs them one thousand pounds sterling.

Colonel John Schuyler, one of the owners, has a deer park five miles round, fenced with cedar logs, five logs high, with blocks of wood between. It contains a variety of land, high and low, woodland and clear. There are a great many deer in it; and he expects in a few years to be able to kill two hundred head a year, which will be a very profitable thing. He has likewise six hundred acres of meadow, all within bank. The mine is not far from Passaic Falls, which I went also to see. They are very curious; the water falls seventy feet perpendicularly, as we were told; but we had nothing to measure with.

It will be agreeable to you to hear, that our subscription goes on with great success, and we suppose will exceed five thousand pounds of our currency. We have bought for the Academy the house that was built for itinerant preaching, which stands on a large lot of ground capable of receiving more buildings to lodge the scholars, if it should come to be a regular college. The house is one hundred feet long and seventy wide, built of brick, very strong, and sufficiently high for three lofty stories. I suppose the building did not cost less than two thousand pounds; but we bought it for seven hundred seventy-five pounds, eighteen shillings, eleven pence, and three farthings; though it will cost us three and perhaps four hundred more to make the partitions and floors, and fit up the rooms. I send you enclosed a copy of our present constitutions; but we expect a charter from our Proprietaries this summer, when they may probably receive considerable alterations. The paper admonishes me that it is time to conclude.

I am, Sir,

Your obliged humble servant,

B. FRANKLIN.

TO CADWALLADER COLDEN.*

Electrical Papers and Experiments. — James Logan.

Philadelphia, 28 June, 1750.

SIR,

I wrote a line to you last post, and sent you some electrical observations and experiments. You formerly had those papers of mine, out of which something has been taken by Mr. Watson, and inserted in the Transactions. If you have forgot the contents of those papers, I am afraid some things in that I last sent you will hardly be understood, as they depend on what went before. I send you herewith my essay towards a new hypothesis of the cause and effects of lightning, &c., of which you may remember some hints in my first electrical minutes. I sent this essay above a twelvemonth since to Dr. Mitchell in London, and have since heard nothing of it, which makes me doubt of its getting to hand. In some late experiments, I have not only frequently fired unwarmed spirits by the electrical stroke, but have even melted small quantities of copper, silver, and gold; and not only melted, but vitrified them, so as to incorporate them with common glass; and this without any sensible heat, which strengthens my supposition, that the melting of metals by lightning may be a cold fusion. Of these experiments I shall shortly write a particular account. I wrote to Mr. Collinson, on reading in the Transactions the accounts, from Italy and Germany, of giving purges, transferring odors, &c. with the electrical effluvia, that I was persuaded they were not true. He since informs me, that Abbé

* This letter was first published in the *American Medical and Philosophical Register*, for January, 1812. — EDITOR.

Nollet, of Paris, who had tried the experiments without success, was lately at the pains to make a journey to Turin, Bologna, and Venice, to inquire into the facts, and see the experiments repeated, imagining they had there some knacks of operating that he was unacquainted with; but, to his great disappointment, found little or no satisfaction; the gentlemen there having been too premature in publishing their imaginations and expectations for real experiments. Please to return me the papers when you have perused them.

My good old friend, Mr. Logan, being about three months since struck with a palsy, continues speechless, though he knows people, and seems in some degree to retain his memory and understanding. I fear he will not recover. Mr. Kalm * is gone towards Canada again, and Mr. Evans† is about to take a journey to Lake Erie, which he intends next week. Mr. Bartram continues well and hearty. I thank you for what you write concerning celestial observations. We are going on with our building for the Academy, and propose to have an observatory on the top; and, as we shall have a mathematical professor, I doubt not but we shall soon be able to send you some observations accurately made.

I am, with great esteem and respect, &c.

B. FRANKLIN.

P. S. If you think it would be agreeable to Mr. Alexander, or any other friend in New York, to peruse these electrical papers, you may return them to me through his hands.

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† A Swedish botanist, who travelled in different parts of America, and afterwards published an account of his travels. — EDITOR.

‡ Lewis Evans, author of "*Geographical, Historical, Political, Philosophical, and Mechanical Essays*," of some other tracts, and of a Map of the Middle Colonies, which was valued as being better than any that had preceded it. — EDITOR.

TO JARED ELIOT.

Inquiries respecting the Mode of Planting Hedges.

Philadelphia, 25 October, 1750.

DEAR SIR,

I ought to have informed you sooner, that we got well home, and should have inquired after your health, as we left you in the hands of a fever. I beg you will excuse the delay, and desire you will remember in my favor the old saying, *They who have much business must have much pardon.* Whenever Mr. Francis and I meet of an evening, we drink your health, among our other New England friends, and he desires to be always respectfully remembered to you.

I am glad to hear you are got well again; but I cannot have the pleasure of seeing you again this year. I will write to Colonel Schuyler, and obtain for you a particular account of his manner of improving his banked grounds; and will also procure for you a specimen of our alum earth, with Mr. Syng's observations on it. In return (for you know there is no trade without returns) I request you to procure for me a particular account of the manner of making a new kind of fence we saw at Southhold, on Long Island, which consists of a bank and hedge. I would know every particular relating to this matter, as the best thickness, height, and slope of the bank; the manner of erecting it, the best time for the work, the best way of planting the hedge, the price of the work to laborers per rod or perch, and whatever may be of use for our information here, who begin in many places to be at a loss for wood to make fence with. We were told at Southhold, that this kind of fencing had been long practised with success at Southampton and other places, on the south side

of the Island, but was new among them. I hear the minister at Southhold is esteemed an ingenious man; perhaps you may know him, and he will at your request favor me with an explicit account of these fences.

The fore part of the summer here was extremely dry, and the grass in many places was burnt up. But we had a good crop of wheat; and, rains coming on about the end of July, we had in August a new spring, the grass sprouting again wonderfully thick and fast, in fields where we thought the very roots had been destroyed. Our grave-diggers said they found the earth hot sensibly at three feet depth, even after these rains; perhaps the great heat below, and the moisture above, occasioned this sudden and profuse vegetation, the whole country being, as it were, one great hot-bed.

I am, with esteem and affection, dear Sir,

Your obliged humble servant,

B. FRANKLIN.

TO JARED ELIOT.

Remarks on Husbandry.

Philadelphia, 10 December, 1751.

DEAR SIR,

The rector of our Academy, Mr. Martin, came over to this country on a scheme for making potash, in the Russian method. He promised me some written directions for you, which expecting daily, I delayed writing, and now he lies dangerously ill of a kind of quinsy. The surgeons have been obliged to open his windpipe, and introduce a leaden pipe for him to breathe through. I fear he will not recover.

I thank you for the merino wool. It is a curiosity. Mr. Roberts promises me some observations on husbandry for you. It is one Mr. Masters, that makes manure of leaves, and not Mr. Roberts. I hope to get the particulars from him soon.

I have a letter from Mr. Collinson, of July 19th, in which he says; "Pray, has Mr. Eliot published any addition to his work? I have Nos. 1 and 2. If I can get ready, I will send some improvements made in the sandy parts of the county of Norfolk. By the way, *it is a great secret*, but it is Mr. Jackson's own drawing up, being experiments made on some of his father's estates in that county; but his name must not be mentioned. I thank you for the fowl meadow grass. I sowed it June 7th, as soon as I received it, but none is yet come up. I don't know how it is, but I never could raise any of your native grasses; and I have had a variety from J. Bartram of curious species."

In another, of September 26th, he says, "I am much obliged to thee for Mr. Eliot's Third Essay. I have sent Maxwell's 'Select Transactions in Husbandry.' If Mr. Eliot has not seen them, they may be very useful to him. I have prevailed on our worthy, learned, and ingenious friend Mr. Jackson to give some dissertations on the husbandry of Norfolk, believing it may be very serviceable to the colonies. He has great opportunities of doing this, being a gentleman of leisure and fortune, being the only son, whose father has great riches and possessions, and resides every year, all the long vacation, at his father's seat in Norfolk. After J. Bartram has perused it, I shall submit how it may be further disposed of, only our friend Eliot should see it soon; for Jackson admires his little Tracts of Husbandry, as well as myself, and it may be of greater service to him and his colony, than to yours." "The fowl meadow

grass has at last made its appearance. Another year we shall judge better of it." Thus far friend Collinson. You may expect the papers in a post or two. If you make any use of them, you will take care not to mention any thing of the author.

The bearer is my son, who desired an opportunity of paying his respects to you in his return from Boston. He went by sea.

They have printed all my electrical papers in England, and sent me a few copies, of which I design to send you one per next post, after having corrected a few errata. I am, dear Sir,

Your most humble servant,

B. FRANKLIN.

P. S. Mr. Martin is dead.

FROM CADWALLADER COLDEN TO B. FRANKLIN.

Concerning Theories of Light.

Coldenham, 20 May, 1752.

DEAR SIR,

I received yours of the 23d of April,* as I was going on board the sloop, on my return home from New York, and could not take the pleasure of reading it before I left that place.

One so much conversant in examining any particular phenomenon, or various phenomena issuing from the same cause, must on every occasion give the pleasure of learning something new to those less conversant in them, as often as the former is pleased to answer any objections made by the latter, or explain the diversities which attend the same phenomenon in different cases or appearances.

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* See this letter in Vol. V. p. 283.

Your conjecture of the electric fluid's taking place in the superior regions of the atmosphere pleases my fancy, as it in some measure confirms what I have advanced in the treatise now in the press, viz. that all the planets, in a greater or less degree, emit light, and indeed, I think, all bodies do. It does not follow, that, because we do not see the light, none is emitted. Some men can see where others are entirely in the dark; and some animals, as owls, bats, &c., see in the dark, and cannot bear so great a degree of light as is necessary for our perceptions.

When you shall see that tract you will have opportunity of judging of the validity of the reasons I advance for an opinion, that light is a substance or being essentially distinct from what we commonly call matter or body; that they have nothing in common between them, except that we consider or conceive both as consisting of quantity, that is, that in the same space there may be a greater or less quantity of either, and that a certain quantity of either may be confined within certain bounds, and consequently have some shape or form. Light has no power of attraction, though it be attracted by resisting matter. The vibrations of a fluid will in no manner explain the phenomena of light, as is very expressly pointed out in Sir Isaac Newton's *Optics;* for example, "Light proceeds always in straight lines, unless diverted by some other thing; for this reason, any opaque body placed between the eye and a luminous body intercepts all the light; but it does not intercept the sound coming from a sonorous body, because sound is conveyed by the vibration of a fluid medium, not by any emission of particles from the sounding body." Again, the separation of the distinct parts of light, which excite in us the different and distinct sensations of colors, and which, once separated,

always remain the same, proves that these sensations cannot be produced by the vibrations of any medium supposed to convey the action of light from the luminous body. I am persuaded, that a careful attention to the phenomena in Sir Isaac's *Optics*, and to his reflections on them, will remove all doubt on this head.

On this occasion I think it proper to observe to you, that, in the treatise before mentioned, what Sir Isaac has proved is generally taken for granted and supposed to be known.

It may be proper likewise to observe to you, that pure light, without any other mixture, makes no impression on any other sense except the sight; that the sense of heat arises from the action of light united with the action of some resisting matter. So, likewise, we have no idea of fire without the union of resisting matter with light.

In considering all quantities or degrees of action or force, whether in ascending or descending, whether in considering them as continually increasing or as continually decreasing, the ratio of comparison must at last come to that of infinity. We have no idea of the absolute force of any thing, only of its comparative force or ratio of its force to that of some other thing. The force of different quantities of light does not arise from the different velocities, (for I suppose all light always moves in the same ratio of velocity,) but from the greater quantity or density of light in the same space.

If the emission of light be not continued but by distinct vibrations or pulses, and an infinitely thin surface of light be thrown off in any finite part of time, suppose in a fifth, then there cannot be any finite or determinable diminution of the light of the sun, or of the diameter of the light in the sun, in any finite time. If the intervals of the vibrations or emissions of infinitely

thin surfaces of light be in an infinitely small part of time, it may take a hundred or a thousand years to diminish the sun's diameter one inch. You will find something of these abstract speculations in the treatise I mention.

I have much reason to apprehend the errors of the press, when I reflect on the manner in which the Indian History has been printed, with such gross errors in things obvious to the meanest understanding. Mr. Dodsley promised to get some person of learning to correct the press. Perhaps the difficulty he meets with in this is the reason that the impression was not completed in the beginning of March, though begun in December.

I shall be exceedingly disappointed if it do not meet with censure. I have laid my account with it, not only with unjust and injurious censure, but that many errors may truly be discovered, and that it will require much correction. It is impossible to avoid errors in things of this nature, more especially in a path not trod in before, and where I had no one to assist me to review and examine the work, a difficulty an author cannot labor under in England.

I have received a copy of the translation of my first piece into High Dutch, with animadversions on it at the end of it, printed at Hamburg and Leipsic, in 1748; but I do not understand one word of them. I find my name often in company with those of very great men; Newton, Leibnitz, and Wolfius. Leibnitz's *Monades* offers mankind a new doctrine, which perhaps you may have seen, and is of great repute in Germany. The animadversions end, "*Magnis lumen excidet ausis*," which, being in Latin, I understood.

The person in the Boston paper, who wants a fuller description of the poke-weed or *phytolacca*, than that

given in the magazine, must have but little skill in botany; for I am confident the description is sufficient for a botanist to distinguish it from any other plant whatsoever. I know that some people have thought that the poke-weed is *mechoachan*; but they, who think so, know little of plants.

CADWALLADER COLDEN.

FROM JOHN PERKINS TO B. FRANKLIN.

*Respecting the Number of Deaths in Boston by Inoculation.**

Boston, 3 August, 1752.

SIR,

This comes to you on account of Dr. Douglass. He desired me to write to you for what you know of the number that died of the inoculation in Philadelphia, telling me he designed to write something on the small-pox shortly. We shall both be obliged to you for a word on this affair.

The chief particulars of our visitation, you have in the public prints. But the less degree of mortality than usual in the common way of infection, seems chiefly owing to the purging method designed to prevent the secondary fever; a method first begun and carried on

* In a letter, dated February 17th, 1752, Dr. Perkins had written as follows; "The small-pox threatens spreading here, and some of our people are coming into a course of tar-water as a preparative. If your doctors have found any benefit by any medical management previous to the common way of infection, I should be glad to know it for the sake of such as are not pleased with inoculation, and for the meliorating of this too. I believe something may be done in this matter, and would promise trials of every thing likely to succeed; though I differ from my brethren in the affair of diet."—EDITOR.

in this town, and with success beyond expectation. We lost one in eleven; but, had we been experienced in this way, at the first coming of the distemper, probably the proportion had been but one in thirteen or fourteen. In the year 1730 we lost one in nine, which is more favorable than ever before with us. The distemper pretty much the same then as now, but some circumstances not so kind this time.

If there be any particulars which you want to know, please to signify what they are, and I shall send them.

The number of our inhabitants decreases. On a strict inquiry, the overseers of the poor find but fourteen thousand one hundred and ninety whites, and one thousand five hundred and forty-four blacks, including those absent, on account of the small-pox, many of whom, it is probable, will never return.

I pass this opportunity without any particulars of my old theme. One thing, however, I must mention, which is, that perhaps my last letters contained something that seemed to militate with your doctrine of the *Origin*, &c. But my design was only to relate the phenomena as they appeared to me. I have received so much light and pleasure from your writings, as to prejudice me in favor of every thing from your hand, and leave me only liberty to observe, and a power of dissenting, when some great probability, might oblige me; and, if at any time that be the case, you will certainly hear of it.

I am, Sir, &c.

JOHN PERKINS.

TO JOHN PERKINS.

Concerning the Small-pox in Philadelphia.

Philadelphia, 13 August, 1752.

SIR,

I received your favor of the 3d instant. Some time last winter I procured from one of our physicians an account of the number of persons inoculated during the five visitations of the small-pox we have had in twenty-two years; which account I sent to Mr. W. V., of your town, and have no copy. If I remember right, the number exceeded eight hundred, and the deaths were but four. I suppose Mr. V. will show you the account, if he ever received it. Those four were all that our doctors allow to have died of the small-pox by inoculation, though I think there were two more of the inoculated who died of the distemper; but the eruptions appearing soon after the operation, it is supposed they had taken the infection before in the common way.

I shall be glad to see what Dr. Douglass may write on the subject. I have a French piece printed at Paris, 1724, entitled, *Observations sur la Saignée du Pied, et sur la Purgation, au Commencement de la Petite Vérole, et Raisons de doubte contre l'Inoculation.* A letter of the Doctor's is mentioned in it. If he or you have it not, and desire to see it, I will send it. Please to favor me with the particulars of your purging method, to prevent the secondary fever.

I am indebted for your preceding letter, but business sometimes obliges one to postpone philosophical amusements. Whatever I have wrote of that kind are really, as they are entitled, but *Conjectures* and *Suppositions;* which ought always to give place, when careful observation militates against them. I own I have too strong

a *penchant* to the building of hypotheses; they indulge my natural indolence. I wish I had more of your patience and accuracy in making observations, on which alone true philosophy can be founded. And, I assure you, nothing can be more obliging to me, than your kind communication of those you make, however they may disagree with my preconceived notions.

I am sorry to hear, that the number of your inhabitants decreases. I some time since wrote a small paper of *Thoughts on the Peopling of Countries*,* which, if I can find, I will send you, to obtain your sentiments. The favorable opinion you express of my writings may, you see, occasion you more trouble than you expected from,

Sir, yours, &c.

B. FRANKLIN.

TO CADWALLADER COLDEN.

Remarks on Mr. Colden's Theory of Light. — Translation of Electrical Papers into French.

Philadelphia, 14 September, 1752.

DEAR SIR,

When I had read your favor of May the 20th, I resolved to read and consider more carefully Sir Isaac Newton's *Optics*, which I have not looked at these many years. I delayed answering, till I should have an opportunity of doing this, but one thing or other has hitherto hindered. In the winter I may possibly have more leisure.

In the mean time I would just mention, that the interposition of a hill between a bell and the ear does

* See this paper in Vol. II. p. 311.

interrupt a great part of the sound, though not all; and we cannot be certain that an opaque body placed between the eye and a luminous object intercepts all the light, since, as you observe, it does not follow that where we see no light there is therefore none existing. What you say of the separation of the distinct parts of light, which, once separated, remain always the same, has more weight with me, and indeed seems conclusive; at least, I see at present nothing to object.

I congratulate you on the prospect you have, of passing the remainder of life in philosophical retirement. I wish for the same, but it seems too distant. I might then more punctually perform my part in the correspondence you honor me with; than which I have none more instructive or agreeable.

Send me, if you please, the translation of your piece into High Dutch. I understand a little of the German language, and will peruse and return it. At present I cannot guess the meaning of the passage you mention. Unless perhaps, as your twentieth section speaks of "a power that neither resists nor moves, and exerts no kind of action of itself, without the concurrence of some other power; so that in the absence of other powers it must be in a perfect inaction," &c., it may be some kind of Dutch wit, and intended to joke that *quietism*, which in Germany is supposed to be very prevalent in Pennsylvania, many of their Quietists having removed hither.

I see by Cave's *Magazine* for May, that they have translated my electrical papers into French, and printed them in Paris. I hope our friend Collinson will procure and send me a copy of the translation. Such things should be done by men skilled in the subject, as well as in the language, otherwise great mistakes are easily made, and the clearest matters rendered obscure and unintelligible.

I am sorry you could not see Mr. Kinnersley's Lectures; they would have pleased you. I send you Mr. Wilson's book, which I just received from London, and think it contains the best directions for the use of the machine, that are extant. When you have done with it, please to return it to,

Dear Sir,

Your most humble servant,

B. FRANKLIN.

FROM CADWALLADER COLDEN TO B. FRANKLIN.

Cause of the Cohesion of the Particles of Matter. — Concerning a Translation of a German Writer's Remarks on Mr. Colden's Philosophical Principles.

Coldenham, 24 October, 1752.

DEAR SIR,

I now send back to you Wilson's *Electricity*, for the use of which I am much obliged. My youngest son, the only one I have with me, hopes to be able to make electrical experiments tolerably well. Mr. Wilson, I think, is on the true scent of the cause of electricity, though it be plain he is not sufficiently informed of the nature of that elastic fluid, which he calls ether, to be able sufficiently to account for the phenomena. In my opinion, some more perfect knowledge of the air, than we have is likewise necessary, and the cause of the cohesion of the parts of bodies, which last has been lately the subject of my meditations. No tolerable account of this, so far as I know, has been given by any philosopher; and, if this can be accounted for from my principles, it will go a great way to confirm them. I hope to let you see something on this head.

At present, I shall only mention one theorem, viz. that the parts or particles of bodies are at a great distance from each other in the ratio of their bulk. To prove this, it is supposed that the solid particles of inert matter are impenetrable by light, or that light cannot be in the same space with matter. Then, since light freely passes through pellucid bodies in straight lines, and in all directions, with a very small diminution of its quantity, the distance between the particles of pellucid bodies must be very great in proportion to their bulk, if the prodigious number of those particles be considered, which must be in a pellucid body of any bulk. This may be easily conceived, by placing a number of points in any order, and considering how they must stop the progress of light in many directions. Again, the number of particles in the same quantity of space in several bodies is as their specific gravity. Now, considering the difference of specific gravity of pellucid bodies, and of any other opaque body, and that it does not arise to a great degree, the parts of all bodies must be at a great distance from each other in proportion to their bulk. Sir Isaac Newton accounts for the cohesion of the parts of bodies, from the stronger attraction in little bodies than in great bodies; but, if this were the cause, the parts of bodies must move together into mutual contact, if some other power do not keep them separated. What I call ether is essentially different from Mr. Wilson's ether, and from that elastic fluid, which I think produces electrical phenomena. Sir Isaac Newton was far from having clear conceptions of what I call ether; though he perceived from the phenomena that some such medium must exist between the several bodies in the universe, and within them between their component parts.

I have at last got the *Remarks* on the first causes

of action in matter well translated by Mr. Harwich, a Lutheran minister, who is well acquainted with the German systems of philosophy, and thereby more capable of making a good translation. I have likewise drawn up an Answer to the Remarks, which I expect may assist you to form a better conception of my principles, and of the truth of them, than what you have already seen. I now send both the Remarks and Answer to Mr. Alexander, and I shall desire him to transmit them to you, if he do not think it necessary to alter any thing in the Answer. In the mean time, I send you the original Remarks in the German language, that you may better judge of the translation when you shall see it.

The Remarks and Answer are chiefly on the metaphysical part of my system. The same subject is treated of in Dr. Johnson's *Noëtica*,* but in a very different manner. I hope from your friendship, that you will give your sentiments without reserve; and I beg that you will take some pains, because I have some distant prospect of being able to explain the phenomena of electricity from my principles, with your assistance. If this can be done, I am persuaded that the

* A treatise entitled, "*Noëtica; or the First Principles of Human Knowledge.*" The author, Dr. Samuel Johnson, was a clergyman of the Episcopal Church, and the first President of King's College in New York. He was distinguished as a scholar, a writer, and a divine. The above work treats of metaphysics and logic, and is remarkable for the clearness of its method, and the acuteness with which some of the difficult topics in mental philosophy are discussed. It was followed by a treatise called "*Ethica; or the First Principles of Moral Philosophy.*" In his Preface the author says; "As the NOËTICA imply all those instructions and means, that relate to the conduct of the understanding in pursuit of the knowledge of truth; so the ETHICA imply all those instructions and means, that relate to the regulation and conduct of our affections, actions, and behaviour in pursuit of the enjoyment of our true and chief good." Both these works were printed by Franklin and Hall, in Philadelphia. — EDITOR.

greatest improvement will thereby be made in the most useful parts of physics. I conceive that fermentations of all sorts arise from electricity, and that the life and vegetation of animals and vegetables arise from fermentation. If so, the knowledge of electricity must give great light in medicine and agriculture. For this reason, I will gladly take pains to remove any difficulties you may have in receiving or conceiving these principles. I have not, as yet, heard that the tract on the "Principles of Action in Matter" is published, though I had the first sheet sent me in January last.

I am, &c.

CADWALLADER COLDEN.

P. S. This having lain by me some days for an opportunity to send it, I have, in that time, seen in the newspapers the account of the electrical kite.* I hope a more perfect and particular account of it will be published, in a manner to preserve it better, and to give it more credit, than it can obtain from a common newspaper. I wish you would attempt some experiments to ascertain whether the electrical fluid can be drawn from fermentary liquors or mixtures. I propose to try; but what may fail with me may succeed with you, as you have such sagacity in contriving proper experiments for any purpose you may have in view. The clouds, before a thunder-gust, and often in the time of it, appear to be in a violent intestine motion or fermentation.

* Alluding to Franklin's celebrated experiment with a kite, to bring lightning from the clouds. See an account of this experiment in Vol. V. p. 175.—EDITOR.

*Physical and Meteorological Observations, Conjectures, and Suppositions.**

READ AT THE ROYAL SOCIETY, JUNE 3D, 1756.

THE particles of air are kept at a distance from each other by their mutual repulsion.

Every three particles, mutually and equally repelling each other, must form an equilateral triangle.

All the particles of air gravitate towards the earth, which gravitation compresses them, and shortens the sides of the triangles; otherwise their mutual repellency would force them to greater distances from each other.

Whatever particles of other matter (not endued with that repellency) are supported in air, must adhere to the particles of air, and be supported by them; for in the vacancies there is nothing they can rest on.

Air and water mutually attract each other. Hence water will dissolve in air, as salt in water.

The specific gravity of matter is not altered by dividing the matter, though the superficies be increased. Sixteen leaden bullets, of an ounce each, weigh as much in water as one of a pound, whose superficies is less.

Therefore, the supporting of salt in water is not owing to its superficies being increased.

A lump of salt, though laid at rest at the bottom of a vessel of water, will dissolve therein, and its parts move every way, till equally diffused in the water; therefore there is a mutual attraction between water and salt. Every particle of water assumes as many of salt as can adhere to it; when more is added, it precipitates, and will not remain suspended.

* The date of this paper is uncertain. Its various parts were probably written at different times. It was sent to Mr. Collinson, with other philosophical papers, and a letter dated November 23d, 1753. See Vol. V. p. 314. — EDITOR.

Water, in the same manner, will dissolve in air, every particle of air assuming one or more particles of water. When too much is added, it precipitates in rain.

But there not being the same contiguity between the particles of air as of water, the solution of water in air is not carried on without a motion of the air, so as to cause a fresh accession of dry particles.

Part of a fluid, having more of what it dissolves, will communicate to other parts that have less. Thus very salt water, coming in contact with fresh, communicates its saltness till all is equal, and the sooner if there is a little motion of the water.

Even earth will dissolve, or mix with air. A stroke of a horse's hoof on the ground, in a hot dusty road, will raise a cloud of dust, that shall, if there be a light breeze, expand every way, till, perhaps, near as big as a common house. It is not by mechanical motion communicated to the particles of dust by the hoof, that they fly so far, nor by the wind, that they spread so wide; but the air near the ground, more heated by the hot dust struck into it, is rarefied and rises, and in rising mixes with the cooler air, and communicates of its dust to it, and it is at length so diffused as to become invisible. Quantities of dust are thus carried up in dry seasons; showers wash it from the air, and bring it down again. For, water attracting it stronger, it quits the air, and adheres to the water.

Air, suffering continual changes in the degrees of its heat, from various causes and circumstances, and, consequently, changes in its specific gravity, must therefore be in continual motion.

A small quantity of fire mixed with water (or degree of heat therein) so weakens the cohesion of its particles, that those on the surface easily quit it, and adhere to the particles of air.

A greater degree of heat is required to break the cohesion between water and air.

Air moderately heated will support a greater quantity of water invisibly than cold air; for its particles, being by heat repelled to a greater distance from each other, thereby more easily keep the particles of water that are annexed to them from running into cohesions that would obstruct, refract, or reflect the light.

Hence, when we breathe in warm air, though the same quantity of moisture may be taken up from the lungs, as when we breathe in cold air, yet that moisture is not so visible.

Water being extremely heated, that is, to the degree of boiling, its particles in quitting it so repel each other, as to take up vastly more space than before, and by that repellency support themselves, expelling the air from the space they occupy. That degree of heat being lessened, they again mutually attract; and having no air particles mixed to adhere to, by which they might be supported and kept at a distance, they instantly fall, coalesce, and become water again.

The water commonly diffused in our atmosphere never receives such a degree of heat from the sun, or other cause, as water has when boiling; it is not, therefore, supported by such heat, but by adhering to air.

Water being dissolved in, and adhering to air, that air will not readily take up oil, because of the mutual repellency between water and oil.

Hence cold oils evaporate but slowly, the air having generally a quantity of dissolved water.

Oil being heated extremely, the air that approaches its surface will be also heated extremely; the water then quitting it, it will attract and carry off oil, which can now adhere to it. Hence the quick evaporation of oil heated to a great degree.

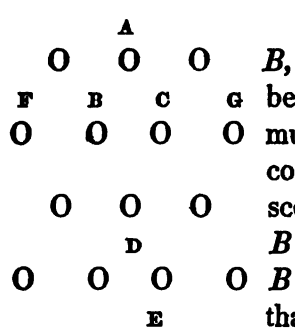
Oil being dissolved in air, the particles to which it adheres will not take up water.

Hence the suffocating nature of air impregnated with burnt grease, as from snuffs of candles and the like. A certain quantity of moisture should be every moment discharged and taken away from the lungs; air that has been frequently breathed, is already overloaded, and, for that reason, can take no more, so will not answer the end. Greasy air refuses to touch it. In both cases suffocation for want of the discharge.

Air will attract and support many other substances.

A particle of air loaded with adhering water, or any other matter, is heavier than before, and would descend.

The atmosphere supposed at rest, a loaded descending particle must act with a force on the particles it passes between, or meets with, sufficient to overcome, in some degree, their mutual repellency, and push them nearer to each other.



Thus, supposing the particles *A*, *B*, *C*, *D*, and the other near them, to be at the distance caused by their mutual repellency (confined by their common gravity), if *A* would descend to *E*, it must pass between *B* and *C*; when it comes between *B* and *C*, it will be nearer to them than before, and must either have pushed them nearer to *F* and *G*, contrary to their mutual repellency, or pass through by a force exceeding its repellency with them. It then approaches *D*, and, to move it out of the way, must act on it with a force sufficient to overcome its repellency with the two next lower particles, by which it is kept in its present situation.

Every particle of air, therefore, will bear any load inferior to the force of these repulsions.

Hence the support of fogs, mists, clouds.

Very warm air, clear, though supporting a very great quantity of moisture, will grow turbid and cloudy on the mixture of a colder air, as foggy, turbid air will grow clear by warming.

Thus the sun, shining on a morning fog, dissipates it; clouds are seen to waste in a sunshiny day.

But cold condenses and renders visible the vapor; a tankard or decanter filled with cold water will condense the moisture of warm, clear air on its outside, where it becomes visible as dew, coalesces into drops, descends in little streams.

The sun heats the air of our atmosphere most near the surface of the earth; for there, besides the direct rays, there are many reflections. Moreover the earth, itself being heated, communicates of its heat to the neighbouring air.

The higher regions, having only the direct rays of the sun passing through them, are comparatively very cold. Hence the cold air on the tops of mountains, and snow on some of them all the year, even in the torrid zone. Hence hail in summer.

If the atmosphere were all of it (both above and below) always of the same temper as to cold or heat, then the upper air would always be *rarer* than the lower, because the pressure on it is less; consequently lighter, and therefore would keep its place.

But the upper air may be more condensed by cold, than the lower air by pressure; the lower more expanded by heat than the upper, for want of pressure. In such case the upper air will become the heavier, the lower the lighter.

The lower region of air being heated and expanded heaves up, and supports for some time the colder, heavier air above, and will continue to support it while the

equilibrium is kept. Thus water is supported in an inverted open glass, while the equilibrium is maintained by the equal pressure upwards of the air below; but, the equilibrium by any means breaking, the water descends on the heavier side, and the air rises into its place.

The lifted heavy, cold air over a heated country, becoming by any means unequally supported, or unequal in its weight, the heaviest part descends first, and the rest follows impetuously. Hence gusts after heats, and hurricanes in hot climates. Hence the air of gusts and hurricanes cold, though in hot climes and seasons; it coming from above.

The cold air descending from above, as it penetrates our warm region full of watery particles, condenses them, renders them visible, forms a cloud thick and dark, overcasting sometimes, at once, large and extensive; sometimes when seen at a distance, small at first, gradually increasing; the cold edge or surface of the cloud condensing the vapors next it, which form smaller clouds that join it, increase its bulk, it descends with the wind and its acquired weight, draws nearer the earth, grows denser with continual additions of water, and discharges heavy showers.

Small black clouds thus appearing in a clear sky, in hot climates, portend storms, and warn seamen to hand their sails.

The earth turning on its axis in about twenty-four hours, the equatorial parts must move about fifteen miles in each minute; in northern and southern latitudes this motion is gradually less to the poles, and there nothing.

If there was a general calm over the face of the globe, it must be by the air's moving in every part as fast as the earth or sea it covers.

He that sails, or rides, has insensibly the same degree

of motion as the ship or coach with which he is connected. If the ship strikes the shore, or the coach stops suddenly, the motion continuing in the man, he is thrown forward. If a man were to jump from the land into a swift-sailing ship, he would be thrown backward (or towards the stern) not having at first the motion of the ship.

He that travels by sea or land towards the equinoctial, gradually acquires motion; from it, loses.

But if a man were taken up from latitude 40 (where suppose the earth's surface to move twelve miles per minute) and immediately set down at the equinoctial, without changing the motion he had, his heels would be struck up, he would fall westward. If taken up from the equinoctial and set down in latitude 40, he would fall eastward.

The air under the equator, and between the tropics, being constantly heated and rarefied by the sun, rises. Its place is supplied by air from northern and southern latitudes, which, coming from parts where the earth and air had less motion, and not suddenly acquiring the quicker motion of the equatorial earth,* appears an east wind blowing westward; the earth moving from west to east, and slipping under the air.

Thus, when we ride in a calm, it seems a wind against us; if we ride with the wind, and faster, even that will seem a small wind against us.

The air rarefied between the tropics, and rising, must flow in the higher region north and south. Before it rose, it had acquired the greatest motion the earth's rotation could give it. It retains some degree of this

* See a paper on this subject, by the late ingenious Mr. Hadley, in the Philosophical Transactions, wherein this hypothesis for explaining the trade-winds first appeared.

motion, and descending in higher latitudes, where the earth's motion is less, will appear a westerly wind, yet tending towards the equatorial parts, to supply the vacancy occasioned by the air of the lower regions flowing thitherwards.

Hence our general cold winds are about northwest; our summer cold gusts the same.

The air in sultry weather, though not cloudy, has a kind of haziness in it, which makes objects at a distance appear dull and indistinct. This haziness is occasioned by the great quantity of moisture equally diffused in that air. When, by the cold wind blowing down among it, it is condensed into clouds, and falls in rain, the air becomes purer and clearer. Hence, after gusts, distant objects appear distinct, their figures sharply terminated.

Extreme cold winds congeal the surface of the earth, by carrying off its fire. Warm winds, afterwards blowing over that frozen surface, will be chilled by it. Could that frozen surface be turned under, and a warmer turned up from beneath it, those warm winds would not be chilled so much.

The surface of the earth is also sometimes much heated by the sun; and such heated surface, not being changed, heats the air that moves over it.

Seas, lakes, and great bodies of water, agitated by the winds, continually change surfaces; the cold surface in winter is turned under by the rolling of the waves, and a warmer turned up; in summer, the warm is turned under, and colder turned up. Hence the more equal temper of sea water, and the air over it. Hence, in winter, winds from the sea seem warm, winds from the land cold. In summer, the contrary.

Therefore the lakes northwest of us,* as they are

* In Pennsylvania.

not so much frozen nor so apt to freeze as the earth, rather moderate than increase the coldness of our winter winds.

The air over the sea, being warmer, and therefore lighter in winter than the air over the frozen land, may be another cause of our general northwest winds, which blow off to sea at right angles from our North American coast; the warm, light sea air rising, the heavy, cold land air pressing into its place.

Heavy fluids, descending, frequently form eddies or whirlpools, as is seen in a funnel, where the water acquires a circular motion, receding every way from a centre, and leaving a vacancy in the middle, greatest above, and lessening downwards, like a speaking trumpet, its big end upwards.

Air descending or ascending may form the same kind of eddies or whirlings, the parts of air acquiring a circular motion, and receding from the middle of the circle by a centrifugal force, and leaving there a vacancy; if descending, greatest above, and lessening downwards; if ascending, greatest below, and lessening upwards, like a speaking-trumpet, standing its big end on the ground.

When the air descends with violence in some places, it may rise with equal violence in others, and form both kinds of whirlwinds.

The air, in its whirling motion receding every way from the centre or axis of the trumpet, leaves there a vacuum, which cannot be filled through the sides, the whirling air, as an arch, preventing; it must then press in at the open ends.

The greatest pressure inwards must be at the lower end, the greatest weight of the surrounding atmosphere being there. The air entering rises within, and carries up dust, leaves, and even heavier bodies, that happen in its way, as the eddy or whirl passes over land.

If it passes over water, the weight of the surrounding atmosphere forces up the water into the vacuity, part of which, by degrees, joins with the whirling air, and adding weight, and receiving accelerated motion, recedes still farther from the centre or axis of the trump, as the pressure lessens; and at last, as the trump widens, is broken into small particles, and so united with air as to be supported by it, and become black clouds at the top of the trump.

Thus these eddies may be whirlwinds at land, water-spouts at sea. A body of water so raised, may be suddenly let fall, when the motion, &c. has not strength to support it, or the whirling arch is broken so as to admit the air; falling in the sea, it is harmless, unless ships happen under it; but, if in the progressive motion of the whirl, it has moved from the sea over the land, and then breaks, sudden, violent, and mischievous torrents are the consequences.

B. FRANKLIN.

FROM JOHN PERKINS TO B. FRANKLIN.

On Water-Spouts.

READ AT THE ROYAL SOCIETY, JUNE 3D, 1756.

Boston, 16 October, 1752.

SIR,

I find by a word or two in your last, that you are willing to be found fault with; which authorizes me to let you know what I am at a loss about in your papers, which is only in the article of the water-spout. I am in doubt whether water in bulk, or even broken into drops, ever ascends into the region of the clouds *per vorticem;*

that is, whether there be, in reality, what I call a direct water-spout. I make no doubt of direct and inverted whirlwinds; your description of them, and the reason of the thing, are sufficient. I am sensible too, that they are very strong, and often move considerable weights. But I have not met with any historical accounts that seem exact enough to remove my scruples concerning the ascent abovesaid.

Descending spouts (as I take them to be) are many times seen, as I take it, in the calms, between the sea and land trade-winds on the coast of Africa. These contrary winds, or diverging, I can conceive may occasion them, as it were by suction, making a breach in a large cloud. But I imagine they have, at the same time, a tendency to hinder any direct or rising spout, by carrying off the lower part of the atmosphere as fast as it begins to rarefy; and yet spouts are frequent here, which strengthens my opinion, that all of them descend.

But however this be, I cannot conceive a force producible by the rarefaction and condensation of our atmosphere, in the circumstances of our globe, capable of carrying water, in large portions, into the region of the clouds. Supposing it to be raised, it would be too heavy to continue the ascent beyond a considerable height, unless parted into small drops; and even then, by its centrifugal force, from the manner of conveyance, it would be flung out of the circle, and fall scattered, like rain.

But I need not expatiate on these matters to you. I have mentioned my objections, and, as truth is my pursuit, shall be glad to be informed. I have seen few accounts of these whirl or eddy winds, and as little of the spouts; and these, especially, lame and poor things to obtain any certainty by. If you know any thing

determinate that has been observed, I shall hope to hear from you; as also of any mistake in my thoughts. I have nothing to object to any part of your suppositions; and, as to that of the trade-winds, I believe nobody can. I am, &c.

JOHN PERKINS.

P. S. The figures in the *Philosophical Transactions* show, by several circumstances, that they all descended, though the relaters seemed to think they took up water.

FROM JOHN PERKINS TO B. FRANKLIN.

Further Remarks on Water-Spouts.

READ AT THE ROYAL SOCIETY, JUNE 24TH, 1756.

Boston, 23 October, 1752.

SIR,

In the enclosed, you have all I have to say of that matter.* It proved longer than I expected, so that I was forced to add a cover to it. I confess it looks like a dispute; but that is quite contrary to my intentions. The sincerity of friendship and esteem were my motives; nor do I doubt your scrupling the goodness of the intention. However, I must confess I cannot tell exactly how far I was acted upon by hopes of better information, in discovering the whole foundation of my opinion, which, indeed, is but an opinion, as I am very much at a loss about the validity of the reasons. I have not been able to differ from you in sentiment concerning any thing else in your *Suppositions*. In the present case I lie open to conviction, and shall be the gainer

* Water-spouts.

when informed. If I am right, you will know that, without my adding any more. Too much said, on a merely speculative matter, is but a robbery committed on practical knowledge. Perhaps I am too much pleased with these dry notions; however, by this you will see that I think it unreasonable to give you more trouble about them, than your leisure and inclination may prompt you to. I am, &c.

SINCE my last, I considered, that, as I had begun with the reasons of my dissatisfaction about the ascent of water in spouts, you would not be unwilling to hear the whole I have to say, and then you will know what I rely upon.

What occasioned my thinking all spouts descend, is, that I found some did certainly do so. A difficulty appeared concerning the ascent of so heavy a body as water, by any force I was apprized of, as probably sufficient. And, above all, a view of Mr. Stuart's portraits of spouts, in the *Philosophical Transactions*.

Some observations on these last will include the chief part of my difficulties.

Mr. Stuart has given us the figures of a number observed by him in the Mediterranean; all with some particulars which make for my opinion, if well drawn. The great spattering, which relaters mention in the water where the spout descends, and which appears in all his drafts, I conceive to be occasioned by drops descending very thick and large into the place.

On the place of this spattering, arises the appearance of a bush, into the centre of which the spout comes down. This bush I take to be formed by a spray, made by the force of these drops, which, being uncommonly large, and descending with unusual force by a stream of wind descending from the cloud with them, increases

the height of the spray; which wind, being repulsed by the surface of the waters, rebounds and spreads; by the first raising the spray higher than it otherwise would go; and by the last making the top of the bush appear to bend outwards, that is, the cloud of spray is forced off from the trunk of the spout, and falls backward.

The bush does the same where there is no appearance of a spout reaching it; and is depressed in the middle, where the spout is expected. This I imagine to be from numerous drops of the spout falling into it, together with the wind I mentioned, by their descent, which beat back the rising spray in the centre.

This circumstance, of the bush bending outwards at the top, seems not to agree with what I call a direct whirlwind, but consistent with the reversed; for a direct one would sweep the bush inwards, if in that case any thing of a bush would appear.

The pillar of water, as they call it, from its likeness, I suppose to be only the end of the spout immersed in the bush, a little blackened by the additional cloud; and, perhaps, appears to the eye beyond its real bigness, by a refraction in the bush, and which refraction may be the cause of the appearance of separation betwixt the part in the bush and that above it. The part in the bush is cylindrical, as it is above; that is, the bigness the same from the top of the bush to the water. Instead of this shape, in case of a whirlwind, it must have been pyramidical.

Another thing remarkable is the curve in some of them; this is easy to conceive, in case of descending parcels of drops through various winds, at least till the cloud condenses so fast as to come down, as it were, *uno rivo*. But it is harder to me to conceive it in the ascent of water, that it should be conveyed along,

secure of not leaking or often dropping through the under side, in the prone part; and, should the water be conveyed so swiftly, and with such force, up into the cloud, as to prevent this, it would, by a natural disposition to move on in a present direction, presently straiten the curve, raising the shoulder very swiftly, till lost in the cloud.

Over every one of Stuart's figures, I see a cloud; I suppose his clouds were first, and then the spout; I do not know whether it be so with all spouts, but suppose it is. Now, if whirlwinds carried up the water, I should expect them in fair weather, but not under a cloud; as is observable of whirlwinds. They come in fair weather, not under the shade of a cloud, nor in the night; since shade cools the air. But, on the contrary, violent winds often descend from the clouds; strong gusts, which occupy small spaces; and, from the higher regions, extensive hurricanes, &c.

Another thing is the appearance of the spout *coming from* the cloud. This I cannot account for on the notion of a direct spout; but, in the real descending one, it is easy. I take it, that the cloud begins first of all to pour out drops at that particular spot, or *foramen;* and, when that current of drops increases, so as to force down wind and vapor, the spout becomes, so far as that goes, opake. I take it, that no clouds drop spouts, but such as make very fast, and happen to condense in a particular spot, which perhaps is coldest, and gives a determination downwards, so as to make a passage through the subjacent atmosphere.

If spouts ascend, it is to carry up the warm rarefied air below, to let down all and any that is colder above; and, if so, they must carry it through the cloud they go into (for that is cold and dense, I imagine,) perhaps far into the higher region, making a wonderful appearance

at a convenient distance to observe it, by the swift rise of a body of vapor above the region of the clouds. But, as this has never been observed in any age, if it be supposable, that is all.

I cannot learn by mariners, that any wind blows towards a spout, more than any other way; but it blows towards a whirlwind for a large distance round.

I suppose there has been no instance of the water of a spout being salt, when coming across any vessel at sea. I suppose, too, that there have been no salt rains; these would make the case clear.

I suppose it is from some unhappy effects of these dangerous creatures of nature, that sailors have a universal dread on them of breaking in their decks, should they come across them.

I imagine spouts, in cold seasons, as Gordon's in the Downs, prove the descent.

Query. Whether there is not always more or less cloud, first, where a spout appears?

Whether they are not, generally, on the borders of trade-winds; and whether this is for, or against me?

Whether there be any credible account of a whirlwind's carrying up all the water in a pool or small pond; as when shoal, and the banks low, a strong gust might be supposed to blow it all out?

Whether a violent tornado, of a small extent, and other sudden and strong gusts, be not winds from above, descending nearly perpendicular; and whether many, that are called whirlwinds at sea, are any other than these, and so might be called air-spouts, if they were objects of sight?

I overlooked, in its proper place, Stuart's No. 11, which is curious for its inequalities; and, in particular, the approach to breaking, which, if it would not be too tedious, I would have observed a little upon, in my own

way, as I think this would argue against the ascent, &c.; but I must pass it, not only for the reason mentioned, but want of room besides.

As to Mr. Stuart's ocular demonstration of the ascent in his great perpendicular spout, the only one it appears in, — I say, as to this, what I have written supposes him mistaken, which yet I am far from asserting.

The force of an airy vortex, having less influence on the solid drops of water, than on the interspersed cloudy vapor, makes the last whirl round swifter, though it descend slower; and this might easily deceive, without great care, the most unprejudiced person.

FROM W. MERCER TO B. FRANKLIN.

Description of a Water-Spout at Antigua.

READ AT THE ROYAL SOCIETY, JUNE 24TH, 1756.

New Brunswick, 11 November, 1752.

SIR,

I am favored with your letter of the 2d instant, and shall with pleasure comply with your request, in describing (as well as my memory serves me) the water-spout I saw at Antigua; and shall think this, or any other service I can do, well repaid, if it contributes to your satisfaction in so curious a disquisition.

I had often seen water-spouts at a distance, and heard many strange stories of them, but never knew any thing satisfactory of their nature or cause, until that which I saw at Antigua; which convinced me that a water-spout is a whirlwind, which becomes visible in all its dimensions by the water it carries up with it.

There appeared not far from the mouth of the harbour of St. John's, two or three water-spouts, one of

which took its course up the harbour. Its progressive motion was slow and unequal, not in a straight line, but, as it were, by jerks or starts. When just by the wharf, I stood about one hundred yards from it. There appeared in the water a circle of about twenty yards diameter, which, to me, had a dreadful, though pleasing appearance. The water in this circle was violently agitated, being whisked about and carried up into the air with great rapidity and noise, and reflected a lustre, as if the sun shined bright on that spot, which was more conspicuous, as there appeared a dark circle around it. When it made the shore, it carried up, with the same violence, shingles, staves,* large pieces of the roofs of houses, &c., and one small wooden house it lifted entire from the foundation on which it stood, and carried it to the distance of fourteen feet, where it settled without breaking or oversetting; and, what is remarkable, though the whirlwind moved from west to east, the house moved from east to west. Two or three negroes and a white woman were killed by the fall of timber, which it carried up into the air and dropped again. After passing through the town, I believe it was soon dissipated; for, except tearing a large limb from a tree, and part of the cover of a sugar-work near the town, I do not remember any farther damage done by it. I conclude, wishing you success in your inquiry, and am, &c.

W. M.

* I suppose shingles, staves, timber, and other lumber might be lying in quantities on the wharf, for sale, as brought from the northern colonies. — B. F.

TO JOHN PERKINS.

Water-spouts and Whirlwinds compared.

READ AT THE ROYAL SOCIETY, JUNE 24TH, 1756.

Philadelphia, 4 February, 1753.

SIR,

I ought to have written to you long since, in answer to yours of October 16th, concerning the water-spout; but business partly, and partly a desire of procuring further information by inquiry among my sea-faring acquaintance, induced me to postpone writing, from time to time, till I am now almost ashamed to resume the subject, not knowing but you may have forgot what has been said upon it.

Nothing certainly can be more improving to a searcher into nature than objections judiciously made to his opinion, taken up, perhaps, too hastily; for such objections oblige him to re-study the point, consider every circumstance carefully, compare facts, make experiments, weigh arguments, and be slow in drawing conclusions. And hence a sure advantage results; for he either confirms a truth, before too slightly supported, or discovers an error, and receives instruction from the objector.

In this view I consider the objections and remarks you sent me, and thank you for them sincerely; but, how much soever my inclinations lead me to philosophical inquiries, I am so engaged in business, public and private, that those more pleasing pursuits are frequently interrupted, and the chain of thought, necessary to be closely continued in such disquisitions, is so broken and disjointed, that it is with difficulty I satisfy myself in any of them; and I am now not much nearer a

conclusion, in this matter of the spout, than when I first read your letter.

Yet, hoping we may in time sift out the truth between us, I will send you my present thoughts, with some observations on your reasons on the accounts in the *Transactions*, and on other relations I have met with. Perhaps, while I am writing, some new light may strike me, for I shall now be obliged to consider the subject with a little more attention.

I agree with you, that, by means of a vacuum in a whirlwind, water cannot be supposed to rise in large masses to the region of the clouds; for the pressure of the surrounding atmosphere could not force it up in a continued body or column, to a much greater height than thirty feet. But, if there really is a vacuum in the centre, or near the axis of whirlwinds, then, I think, water may rise in such vacuum to that height, or to a less height, as the vacuum may be less perfect.

I had not read Stuart's account, in the *Transactions*, for many years, before the receipt of your letter, and had quite forgot it; but now, on viewing his drafts, and considering his descriptions, I think they seem to favor *my hypothesis;* for he describes and draws columns of water, of various heights, terminating abruptly at the top, exactly as water would do, when forced up by the pressure of the atmosphere into an exhausted tube.

I must, however, no longer call it *my hypothesis*, since I find Stuart had the same thought, though somewhat obscurely expressed, where he says, "he imagines this phenomenon may be solved by suction (improperly so called), or rather pulsion, as in the application of a cupping-glass to the flesh, the air being first voided by the kindled flax."

In my paper, I supposed a whirlwind and a spout to be the same thing, and to proceed from the same cause;

the only difference between them being, that the one passes over land, the other over water. I find also in the *Transactions*, that M. de la Pryme was of the same opinion; for he there describes two spouts, as he calls them, which were seen at different times, at Hatfield, in Yorkshire, whose appearances in the air were the same with those of the spouts at sea, and effects the same with those of real whirlwinds.

Whirlwinds have generally a progressive, as well as a circular motion; so had what is called the spout, at Topsham (see the account of it in the *Transactions*), which also appears, by its effects described, to have been a real whirlwind. Water-spouts have, also, a progressive motion; this is sometimes greater, and sometimes less; in some violent, in others barely perceivable. The whirlwind at Warrington continued long in Acrement Close.

Whirlwinds generally arise after calms and great heats; the same is observed of water-spouts, which are therefore most frequent in the warm latitudes. The spout that happened in cold weather, in the Downs, described by Mr. Gordon in the *Transactions*, was, for that reason, thought extraordinary; but he remarks withal, that the weather, though cold when the spout appeared, was soon after much colder; as we find it, commonly, less warm after a whirlwind.

You agree, that the wind blows every way towards a whirlwind, from a large space round. An intelligent whaleman, of Nantucket, informed me, that three of their vessels, which were out in search of whales, happening to be becalmed, lay in sight of each other, at about a league distance, if I remember right, nearly forming a triangle; after some time, a water-spout appeared near the middle of the triangle, when a brisk breeze of wind sprung up, and every vessel made sail;

and then it appeared to them all, by the setting of the sails, and the course each vessel stood, that the spout was to the leeward of every one of them; and they all declared it to have been so, when they happened afterwards in company, and came to confer about it. So that in this particular likewise, whirlwinds and water-spouts agree.

But, if that which appears a water-spout at sea, does sometimes, in its progressive motion, meet with and pass over land, and there produce all the phenomena and effects of a whirlwind, it should thence seem still more evident, that a whirlwind and a spout are the same. I send you, herewith, a letter from an ingenious physician of my acquaintance, which gives one instance of this, that fell within his observation.

A fluid, moving from all points horizontally, towards a centre, must, at that centre, either ascend or descend. Water being in a tub, if a hole be opened in the middle of the bottom, will flow from all sides to the centre, and there descend in a whirl. But air, flowing on and near the surface of land or water, from all sides towards a centre, must at that centre ascend, the land or water hindering its descent.

If these concentring currents of air be in the upper region, they may indeed descend in the spout or whirlwind; but then, when the united current reached the earth or water, it would spread, and, probably, blow every way from the centre. There may be whirlwinds of both kinds, but, from the commonly observed effects, I suspect the rising one to be the most common; when the upper air descends, it is, perhaps, in a greater body extended wider, as in our thunder-gusts, and without much whirling; and, when air descends in a spout, or whirlwind, I should rather expect it would press the roof of a house *inwards*, or force *in* the tiles, shingles,

or thatch, force a boat down into the water, or a piece of timber into the earth, than that it would lift them up, and carry them away.

It has so happened, that I have not met with any accounts of spouts, that certainly descended; I suspect they are not frequent. Please to communicate those you mention. The apparent dropping of a pipe from the clouds towards the earth or sea, I will endeavour to explain hereafter.

The augmentation of the cloud, which, as I am informed, is generally, if not always the case, during a spout, seems to show an ascent, rather than a descent, of the matter of which such cloud is composed; for a descending spout, one would expect, should diminish a cloud. I own, however, that cold air descending, may, by condensing the vapors in a lower region, form and increase clouds; which, I think, is generally the case in our common thunder-gusts, and, therefore, do not lay great stress on this argument.

Whirlwinds and spouts are not always, though most commonly, in the day time. The terrible whirlwind, which damaged a great part of Rome, June 11th, 1749, happened in the night of that day. The same was supposed to have been first a spout, for it is said to be beyond doubt, that it gathered in the neighbouring sea, as it could be tracked from Ostia to Rome. I find this in Père Boscovich's account of it, as abridged in the *Monthly Review* for December, 1750.

In that account, the whirlwind is said to have appeared as a very black, long, and lofty cloud, discoverable, notwithstanding the darkness of the night, by its continually lightning or emitting flashes on all sides, pushing along with a surprising swiftness, and within three or four feet of the ground. Its general effects on houses were, stripping off the roofs, blowing away chim-

neys, breaking doors and windows, *forcing up the floors, and unpaving the rooms*, (some of these effects seem to agree well with a supposed vacuum in the centre of the whirlwind,) and the very rafters of the houses were broken and dispersed, and even hurled against houses at a considerable distance, &c.

It seems, by an expression of Père Boscovich's, as if the wind blew from all sides towards the whirlwind; for, having carefully observed its effects, he concludes of all whirlwinds, "that their motion is circular, and their action attractive."

He observes, on a number of histories of whirlwinds, &c., "that a common effect of them is, to carry up into the air tiles, stones, and animals themselves, which happen to be in their course, and all kinds of bodies unexceptionably, throwing them to a considerable distance, with great impetuosity."

Such effects seem to show a rising current of air.

I will endeavour to explain my conceptions of this matter by figures, representing a plan, and an elevation of a spout or whirlwind.

I would only first beg to be allowed two or three positions, mentioned in my former paper.

1. That the lower region of air is often more heated, and so more rarefied, than the upper; consequently, specifically lighter. The coldness of the upper region is manifested by the hail, which sometimes falls from it in a hot day.

2. That heated air may be very moist, and yet the moisture so equally diffused and rarefied, as not to be visible, till colder air mixes with it, when it condenses, and becomes visible. Thus our breath, invisible in summer, becomes visible in winter.

Now let us suppose a tract of land, or sea, of perhaps sixty miles square, unscreened by clouds, and

unfanned by winds, during great part of a summer's day, or, it may be, for several days successively, till it is violently heated, together with the lower region of air in contact with it, so that the said lower air becomes specifically lighter than the superincumbent higher region of the atmosphere, in which the clouds commonly float; let us suppose, also, that the air surrounding this tract has not been so much heated during those days, and, therefore, remains heavier. The consequence of this should be, as I conceive, that the heated, lighter air, being pressed on all sides, must ascend, and the heavier descend; and, as this rising cannot be in all parts, or the whole area of the tract at once, for that would leave too extensive a vacuum, the rising will begin precisely in that column that happens to be the lightest, or most rarefied; and the warm air will flow horizontally from all points to this column, where the several currents meeting, and joining to rise, a whirl is naturally formed, in the same manner as a whirl is formed in the tub of water, by the descending fluid flowing from all sides of the tub, to the hole in the centre.

And, as the several currents arrive at this central rising column with a considerable degree of horizontal motion, they cannot suddenly change it to a vertical motion; therefore as they gradually, in approaching the whirl, decline from right to curve, or circular lines, so, having joined the whirl, they *ascend* by a spiral motion, in the same manner as the water *descends* spirally, through the hole in the tub beforementioned.

Lastly, as the lower air, and nearest the surface, is most rarefied by the heat of the sun, that air is most acted on by the pressure of the surrounding cold and heavy air, which is to take its place; consequently its motion towards the whirl is swiftest, and so the force of the lower part of the whirl, or trump, strongest, and

the centrifugal force of its particles greatest; and hence, the vacuum round the axis of the whirl should be greatest near the earth or sea, and be gradually diminished as it approaches the region of the clouds, till it ends in a point, as at *P*, in Figure 2, Plate IX., forming a long and sharp cone.

In Figure 1, which is a plan or ground-plat of a whirlwind, the circle *V* represents the central vacuum.

Between *a a a a* and *b b b b*, I suppose a body of air, condensed strongly, by the pressure of the currents moving towards it from all sides without, and by its centrifugal force from within, moving round with prodigious swiftness, (having, as it were, the momenta of all the currents, ——> ——> ——> ——>, united in itself), and with a power equal to its swiftness and density.

It is this whirling body of air between *a a a a* and *b b b b* that rises spirally; by its force it tears buildings to pieces, twists up great trees by the roots, &c., and, by its spiral motion, raises the fragments so high, till the pressure of the surrounding and approaching currents, diminishing, can no longer confine them to the circle, or their own centrifugal force, increasing, grows too strong for such pressure, when they fly off in tangent lines, as stones out of a sling, and fall on all sides, and at great distances.

If it happens at sea, the water under and between *a a a a* and *b b b b* will be violently agitated and driven about, and parts of it raised with the spiral current, and thrown about so as to form a bush-like appearance.

This circle is of various diameters, sometimes very large.

If the vacuum passes over water, the water may rise in it, in a body or column, to near the height of thirty-two feet.

If it passes over houses, it may burst their windows or walls outwards, pluck off the roofs, and pluck up the floors, by the sudden rarefaction of the air contained within such buildings; the outward pressure of the atmosphere being suddenly taken off. So the stopped bottle of air bursts under the exhausted receiver of the air-pump.

Figure 2 is to represent the elevation of a water-spout, wherein I suppose *P P P* to be the cone, at first a vacuum, till *W W*, the rising column of water, has filled so much of it. *S S S S*, the spiral whirl of air, surrounding the vacuum, and continued higher in a close column after the vacuum ends in the point *P*, till it reaches the cool region of the air. *B B*, the bush, described by Stuart, surrounding the foot of the column of water.

Now, I suppose, this whirl of air will, at first, be as invisible as the air itself, though reaching in reality from the water to the region of cool air, in which our low summer thunder-clouds commonly float; but presently it will become visible at its extremities. *At its lower end*, by the agitation of the water under the whirling part of the circle, between *P* and *S*, forming Stuart's bush, and by the swelling and rising of the water in the beginning vacuum, which is at first a small, low, broad cone, whose top gradually rises and sharpens, as the force of the whirl increases. *At its upper end* it becomes visible, by the warm air brought up to the cooler region, where its moisture begins to be condensed into thick vapor by the cold, and is seen first at *A*, the highest part, which being now cooled, condenses what rises next at *B*, which condenses that at *C*, and that condenses what is rising at *D*, the cold operating by the contact of the vapors faster in a right line downwards, than the vapors themselves can climb

in a spiral line upwards; they climb, however, and, as by continual addition they grow denser, and consequently their centrifugal force greater, and being risen above the concentrating currents that compose the whirl, fly off, spread, and form a cloud.

It seems easy to conceive, how, by this successive condensation from above, the spout appears to drop or descend from the cloud, though the materials of which it is composed are all the while ascending.

The condensation of the moisture, contained in so great a quantity of warm air as may be supposed to rise in a short time in this prodigiously rapid whirl, is, perhaps, sufficient to form a great extent of cloud, though the spout should be over land, as those at Hatfield; and, if the land happens not to be very dusty, perhaps the lower part of the spout will scarce become visible at all; though the upper, or what is commonly called the descending part, be very distinctly seen.

The same may happen at sea, in case the whirl is not violent enough to make a high vacuum, and raise the column, &c. In such case, the upper part *A B C D* only will be visible, and the bush perhaps below.

But, if the whirl be strong, and there be much dust on the land, and the column *W W* be raised from the water, then the lower part becomes visible, and sometimes even united to the upper part. For the dust may be carried up in the spiral whirl, till it reach the region where the vapor is condensed, and rise with that even to the clouds; and the friction of the whirling air, on the sides of the column *W W*, may detach great quantities of its water, break it into drops, and carry them up in the spiral whirl, mixed with the air; the heavier drops may indeed fly off, and fall in a shower, round the spout; but much of it wil. be broken into vapor, yet visible; and thus, in both cases, by dust at land,

and by water at sea, the whole tube may be darkened and rendered visible.

As the whirl weakens, the tube may (in appearance) separate in the middle; the column of water subsiding, and the superior condensed part drawing up to the cloud. Yet still the tube or whirl of air, may remain entire, the middle only becoming invisible, as not containing visible matter.

Dr. Stuart says, "It was observable of all the spouts he saw, but more perceptible of the great one, that towards the end it began to appear like a hollow canal, only black in the borders, but white in the middle; and, though at first it was altogether black and opake, yet now one could very distinctly perceive the sea water to fly up along the middle of this canal, as smoke up a chimney."

And Dr. Mather, describing a whirlwind, says, "A thick, dark, small cloud arose, with a pillar of light in it, of about eight or ten feet diameter, and passed along the ground in a tract not wider than a street, horribly tearing up trees by the roots, blowing them up in the air like feathers, and throwing up stones of great weight to a considerable height in the air," &c.

These accounts, the one of water-spouts, the other of a whirlwind, seem in this particular to agree; what one gentleman describes as a tube, black in the borders, and white in the middle, the other calls a black cloud, with a pillar of light in it; the latter expression has only a little more of the *marvellous*, but the thing is the same; and it seems not very difficult to understand. When Dr. Stuart's spouts were full charged, that is, when the whirling pipe of air was filled between *a a a a* and *b b b b*, Figure 1, with quantities of drops, and vapor torn off from the column *W W*, Figure 2, the whole was rendered so dark as that it could not be

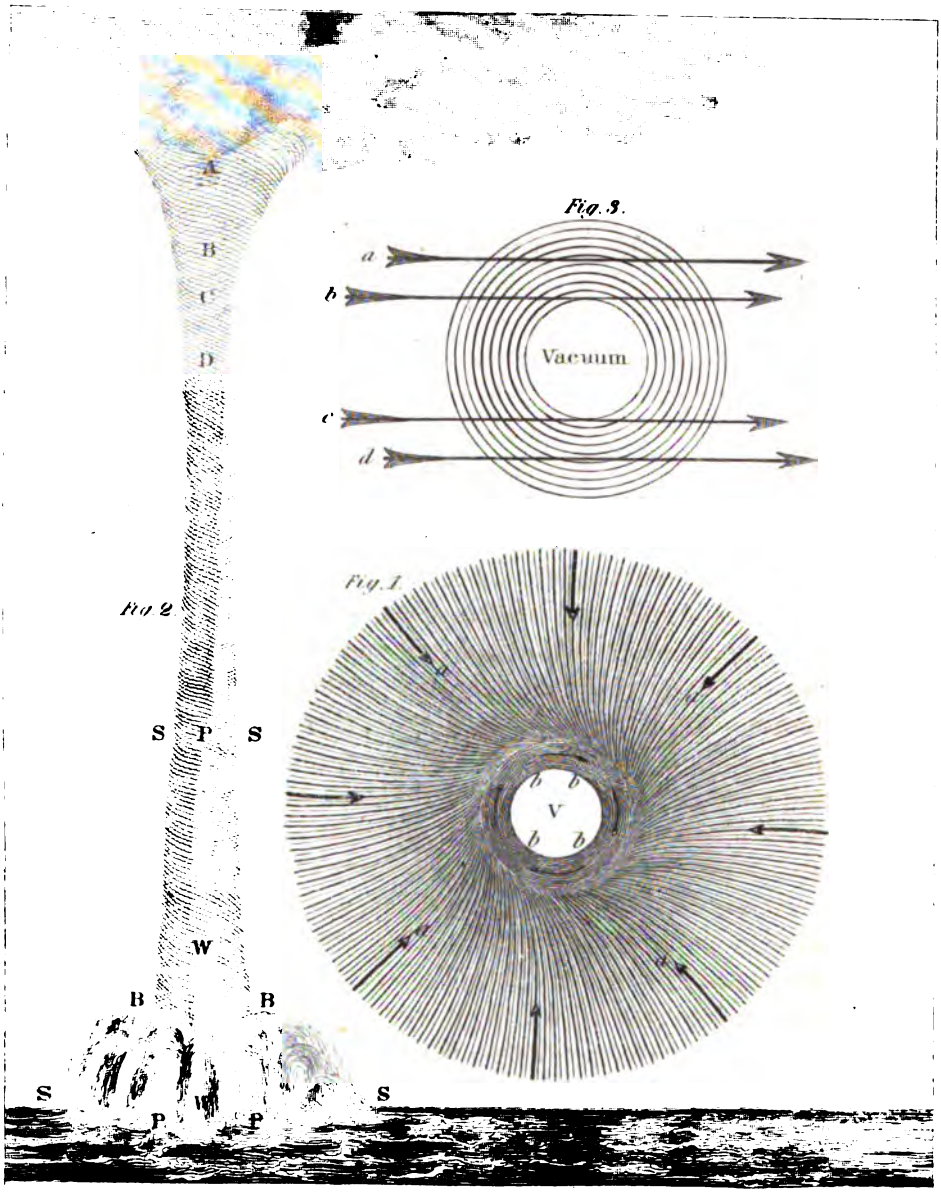
seen through, nor the spiral ascending motion discovered; but when the quantity ascending lessened, the pipe became more transparent, and the ascending motion visible. For, by inspection of the figure, [Fig. 3,] representing a section of our spout, with the vacuum in the middle, it is plain, that, if we look at such a hollow pipe, in the direction of the arrows, and suppose opake particles to be equally mixed in the space between the two circular lines, both the part between the arrows *a* and *b*, and that between the arrows *c* and *d*, will appear much darker than that between *b* and *c*, as there must be many more of those opake particles in the line of vision, across the sides, than across the middle. It is thus, that a hair in a microscope evidently appears to be a pipe, the sides showing darker than the middle. Dr. Mather's whirl was probably filled with dust, the sides were very dark, but, the vacuum within rendering the middle more transparent, he calls it a pillar of light.

It was in this more transparent part between *b* and *c*, that Stuart could see the spiral motion of the vapors, whose lines on the nearest and farthest side of the transparent part crossing each other, represented smoke ascending in a chimney; for, the quantity being still too great in the line of sight through the sides of the tube, the motion could not be discovered there, and so they represented the solid sides of the chimney.

When the vapors reach in the pipe from the clouds near to the earth, it is no wonder now to those who understand electricity, that flashes of lightning should descend by the spout, as in that of Rome.

But you object, If water may be thus carried into the clouds, why have we not salt rains? The objection is strong and reasonable, and I know not whether I can answer it to your satisfaction. I never heard but of one salt rain, and that was where a spout passed pretty near

WATER SPOUT.



a ship; so I suppose it to be only the drops thrown off from the spout, by the centrifugal force (as the birds were at Hatfield), when they had been carried so high as to be above, or to be too strongly centrifugal for, the pressure of the concurring winds surrounding it. And, indeed, I believe there can be no other kind of salt rain; for it has pleased the goodness of God so to order it, that the particles of air will not attract the particles of salt, though they strongly attract water.

Hence, though all metals, even gold, may be united with air, and rendered volatile, salt remains fixed in the fire, and no heat can force it up to any considerable height, or oblige the air to hold it. Hence, when salt rises, as it will a little way, into air with water, there is instantly a separation made; the particles of water adhere to the air, and the particles of salt fall down again, as if repelled and forced off from the water by some power in the air; or, as some metals, dissolved in a proper menstruum, will quit the solvent when other matter approaches, and adhere to that, so the water quits the salt, and embraces the air; but air will not embrace the salt, and quit the water, otherwise our rains would indeed be salt, and every tree and plant on the face of the earth be destroyed, with all the animals that depend on them for subsistence. He who hath proportioned and given proper qualities to all things, was not unmindful of this. Let us adore HIM with praise and thanksgiving!

By some accounts of seamen, it seems the column of water, *W W*, sometimes falls suddenly; and if it be, as some say, fifteen or twenty yards diameter, it must fall with great force, and they may well fear for their ships. By one account, in the *Transactions*, of a spout that fell at Colne in Lancashire, one would think the column is sometimes lifted off from the water, and

carried over land, and there let fall in a body; but this, I suppose, happens rarely.

Stuart describes his spouts as appearing no bigger than a mast, and sometimes less; but they were seen at a league and a half distance.

I think I formerly read in Dampier, or some other voyager, that a spout, in its progressive motion, went over a ship becalmed on the coast of Guinea, and first threw her down on one side, carrying away her fore-mast, then suddenly whipped her up, and threw her down on the other side, carrying away her mizen-mast, and the whole was over in an instant. I suppose the first mischief was done by the fore side of the whirl, the latter by the hinder side, their motion being contrary.

I suppose a whirlwind, or spout, may be stationary, when the concurring winds are equal; but if unequal, the whirl acquires a progressive motion, in the direction of the strongest pressure.

When the wind that gives the progressive motion becomes stronger below than above, or above than below, the spout will be bent, and, the cause ceasing, straighten again.

Your queries, towards the end of your paper, appear judicious, and worth considering. At present I am not furnished with facts sufficient to make any pertinent answer to them; and this paper has already a sufficient quantity of conjecture.

Your manner of accommodating the accounts to your hypothesis of descending spouts is, I own, ingenious, and perhaps that hypothesis may be true. I will consider it farther; but, as yet, I am not satisfied with it, though hereafter I may be.

Here you have my method of accounting for the principal phenomena, which I submit to your candid examination.

And, as I now seem to have almost written a book, instead of a letter, you will think it high time I should conclude; which I beg leave to do, with assuring you, that I am, Sir, &c.

B. FRANKLIN.

TO JAMES BOWDOIN.

Relating to the Transit of Mercury in 1753.

Philadelphia, 28 February, 1753.

DEAR SIR,

The enclosed is a copy of a letter and some papers I received lately from a friend, of which I have struck off fifty copies by the press, to distribute among my ingenious acquaintance in North America, hoping some of them will make the observations proposed. The improvement of geography and astronomy is the common concern of all polite nations, and, I trust, our country will not miss the opportunity of sharing in the honor to be got on this occasion. The French originals are despatched by express overland to Quebec. I doubt not but you will do what may lie in your power, to promote the making these observations in New England, and that we may not be excelled by the American French, either in diligence or accuracy. We have here a three-foot reflecting telescope, and other proper instruments; and intend to observe at our Academy, if the weather permit. You will see, by our Almanac, that we have had this transit under consideration before the arrival of these French letters.*

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* The paper alluded to, of which fifty copies were struck off for distribution, was entitled, "*Letters relating to a Transit of Mercury over the*

Dr. Colden's book was printed in England last summer, but not to be published till the meeting of Parliament. I have one copy, however, which I purpose shortly to send you.

With great esteem and respect, I am, Sir,

Your most humble servant,

B. FRANKLIN.

TO JARED ELIOT.

Properties of Water. — Notice of the Author's Writings in France.

Philadelphia, 12 April, 1753.

DEAR SIR,

I received your favor of March 26th, and thank you for communicating to me the very ingenious letter from your friend, Mr. Todd, with whom, if it may be agreeable to him, I would gladly entertain a correspondence. I shall consider his objections till next post.

I thank you for your hint concerning the word *adhesion*, which should be defined. When I speak of particles of water *adhering* to particles of air, I mean not

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Sun, which is to happen May 6th, 1753." It consisted of four large folio pages, and contained all the information necessary to enable a person little skilled in astronomy to observe the transit. A manuscript drawing was also attached to each copy, showing the line in which Mercury would pass over the sun's disc.

The translation of these papers was made in New York under the direction of Mr. James Alexander, who sent them to Franklin. The French astronomers were desirous, that observations of the transit should be taken at Quebec. M. de Lisle, of the Academy of Sciences, drew up a memorial containing instructions for the purpose. This memorial, with letters from M. La Gallissonière, dated at Paris, October 10th, 1752, was sent unsealed to the governor of New York, with a request that they might be forwarded over land to Quebec. The governor put the papers into the hands of Mr. Alexander, who caused a translation to be made.

a firm adhesion, but a loose one, like that of a drop of water to the end of an icicle before freezing. The firm adhesion is after it is frozen.

I conceive that the original constituent particles of water are perfectly *hard*, *round*, and *smooth*. If so, there must be interstices, and yet the mass incompressible. A box filled with small shot has many interstices, and the shot may be compressed because they are not perfectly hard. If they were, the interstices would remain the same, notwithstanding the greatest pressure, and would admit sand, as water admits salt.

Our vessel, named the Argo, is gone for the northwest passage; and the captain has borrowed my Journals of the last voyage, except one volume of a broken set, which I send you. I enclose a letter from our friend, Mr. Collinson, and am promised some speltz, which I shall send per next post.

The originals were then despatched to Quebec. To the translation, which was printed and distributed by Franklin, was prefixed a long and interesting letter from Mr. Alexander on the subject of the transit. He also communicated a copy of all the papers to Cadwallader Colden.

In a letter to Franklin, which accompanied the manuscripts, Mr. Alexander said; "It would be a great honor to our young colleges in America, if they should forthwith prepare themselves for the observation, and make it, which I doubt not they will do, if they are put in mind of it, and of its great importance to astronomy. The missing of that one observation cannot be retrieved for two hundred and fifty years. You have on so many occasions demonstrated your love of literature and the good of mankind in general, that I thought no person so proper as yourself to think of the ways and means of persuading these colleges to prepare themselves, and in order thereto you may make what use you please of the papers herewith sent."

Mr. Bowdoin said, in reply to the letter in the text; "By the post I received your favor, enclosing several printed letters relating to the transit of Mercury over the sun. A gentleman here, who is provided with the proper instruments, and well skilled in astronomy, intends to make the necessary observations; to whom, as well as to several others, I shall communicate said letters. The rarity of the appearance, and the improvement in geography and astronomy to be expected from it, will induce them, I hope, to make their observations with such accuracy as shall answer the end proposed." — EDITOR.

The *Tatler* tells us of a girl, who was observed to grow suddenly proud, and none could guess the reason, till it came to be known that she had got on a pair of new silk garters. Lest you should be puzzled to guess the cause, when you observe any thing of the kind in me, I think I will not hide my new garters under my petticoats, but take the freedom to show them to you, in a paragraph of our friend Collinson's last letter, viz. — But I ought to mortify, and not indulge, this vanity; I will not transcribe the paragraph, yet I cannot forbear.

"If any of thy friends," says Peter, "should take notice that thy head is held a little higher up than formerly, let them know; when the grand monarch of France strictly commands the Abbé Mazéas to write a letter in the politest terms to the Royal Society, to return the King's thanks and compliments in an express manner to Mr. Franklin of Pennsylvania, for his useful discoveries in electricity, and application of the pointed rods to prevent the terrible effects of thunder-storms, I say, after all this, is not some allowance to be made, if thy crest is a little elevated? There are four letters containing very curious experiments on thy doctrine of points, and its verification, which will be printed in the new Transactions. I think, now I have stuck a feather in thy cap, I may be allowed to conclude in wishing thee long to wear it. Thine, P. Collinson."

On reconsidering this paragraph, I fear I have not so much reason to be proud as the girl had; for a feather in the cap is not so useful a thing, or so serviceable to the wearer, as a pair of good silk garters. The pride of man is very differently gratified; and, had his Majesty sent me a marshal's staff, I think I should scarce have been so proud of it, as I am of your esteem, and of subscribing myself, with sincerity, dear Sir,

Your affectionate friend and humble servant,

B. Franklin.

FROM JOHN PERKINS TO B. FRANKLIN.

Shooting Stars.

READ AT THE ROYAL SOCIETY, JULY 8TH, 1756.

Boston, 14 May, 1753.

SIR,

I received your letter of April last, and thank you for it. Several things in it make me at a loss which side the truth lies on, and determine me to wait for farther evidence.

As to shooting stars, as they are called, I know very little, and hardly know what to say. I imagine them to be passes of electric fire from place to place in the atmosphere, perhaps occasioned by accidental pressures of a non-electric, circumambient fluid, and so by propulsion, or allicited by the circumstance of a distant quantity *minus* electrified, which it shoots to supply, and becomes apparent by its contracted passage through a non-electric medium. Electric fire in our globe is always in action, sometimes ascending, descending, or passing from region to region. I suppose it avoids too dry air, and therefore we never see these shoots ascend. It always has freedom enough to pass down unobserved, but, I imagine, not always so, to pass to distant climes and meridians less stored with it.

The shoots are sometimes all one way, which, in the last case, they should be.

Possibly there may be collections of particles in our atmosphere, which gradually form, by attraction, either similar ones *per se*, or dissimilar particles, by the intervention of others. But, then, whether they shoot or explode of themselves, or by the approach of some suitable foreign collection, accidentally brought near by the usual commotions and interchanges of our atmosphere,

especially when the higher and lower regions intermix, before change of winds and weather, I leave.

I believe I have now said enough of what I know nothing about. If it should serve for your amusement, or any way oblige you, it is all I aim at; and I shall, at your desire, be always ready to say what I think, as I am sure of your candor. I am, &c.

J. PERKINS.

FROM JOHN PERKINS TO B. FRANKLIN.

Water-Spouts and Whirlwinds.

READ AT THE ROYAL SOCIETY, JULY 8TH, 1756.

SPOUTS have been generally believed ascents of water from below to the region of the clouds, and whirlwinds the means of conveyance. The world has been very well satisfied with these opinions, and prejudiced with respect to any observations about them. Men of learning and capacity have had many opportunities in passing those regions where these phenomena were most frequent, but seem industriously to have declined any notice of them, unless to escape danger, as a matter of mere impertinence in a case so clear and certain as their nature and manner of operation are taken to be. Hence it has been very difficult to get any tolerable accounts of them. None, but those they fell near, can inform us any thing to be depended on; three or four such instances follow, where the vessels were so near, that their crews could not avoid knowing something remarkable with respect to the matters in question.

Captain John Wakefield, junior, passing the Straits of Gibraltar, had one fall by the side of his ship; it came down of a sudden, as they think, and all agree the descent was certain.

Captain Langstaff, on a voyage to the West Indies, had one come across the stern of his vessel, and pass away from him. The water came down in such quantity, that the present Captain Melling, who was then a common sailor at helm, says it almost drowned him, running into his mouth, nose, ears, &c., and adds, that it tasted perfectly fresh.

One passed by the side of Captain Howland's ship, so near that it appeared pretty plain that the water descended from first to last.

Mr. Robert Spring was so near one, in the Straits of Malacca, that he could perceive it to be a small very thick rain.

All these assure me, that there was no wind drawing towards them, nor have I found any others that have observed such a wind.

It seems plain, by these few instances, that whirlwinds do not always attend spouts; and that the water really descends in some of them. But the following consideration, in confirmation of this opinion, may, perhaps, render it probable that all the spouts are descents.

It seems unlikely that there should be two sorts of spouts, one ascending and the other descending.

It has not yet been proved, that any one spout ever ascended. A specious appearance is all that can be produced in favor of this; and those, who have been most positive about it, were at more than a league's distance when they observed, as Stuart and others, if I am not mistaken. However, I believe it impossible to be certain whether water ascends or descends at half the distance.

It may not be amiss to consider the places where they happen most. These are such as are liable to calms from departing winds on both sides, as on the borders of the equinoctial trade, calms on the coast of

Guinea, in the Straits of Malacca, &c., places where the under region of the atmosphere is drawn off horizontally. I think they do not come where the calms are without departing winds; and I take the reason to be, that such places, and places where winds blow towards one another, are liable to whirlwinds, or other ascents of the lower region, which I suppose contrary to spouts. But the former are liable to descents, which I take to be necessary to their production. Agreeably to this, it seems reasonable to believe, that any Mediterranean sea should be more subject to spouts than others. The sea, usually so called, is so. The Straits of Malacca are. Some large gulfs may probably be so, in suitable latitudes; so the Red Sea, &c.; and all for this reason, that the heated lands on each side draw off the under region of the air, and make the upper descend, whence sudden and wonderful condensations may take place, and make these descents.

It seems to me, that the manner of their appearance and procedure, favor the notion of a descent.

More or less of a cloud, as I am informed, always appears over the place first; then a spattering on the surface of the water below; and, when this is advanced to a considerable degree, the spout emerges from the cloud, and descends, and that, if the causes are sufficient, down to the places of spattering, with a roaring in proportion to the quantity of the discharge; then it abates, or stops, sometimes more gradually, sometimes more suddenly.

I must observe a few things on these particulars, to show how I think they agree with my hypothesis.

The preceding cloud over the place shows condensation, and consequently tendency downwards, which therefore must naturally prevent any ascent. Besides that, so far as I can learn, a whirlwind never comes under a cloud but in a clear sky.

The spattering may be easily conceived to be caused by a stream of drops, falling with great force on the place, imagining the spout to begin so, when a sudden and great condensation happens in a contracted space, as the Ox-Eye, on the coast of Guinea.

The spout appearing to descend from the cloud seems to be, by the stream of nearly contiguous drops bringing the air into consent, so as to carry down a quantity of the vapor of the cloud; and the pointed appearance it makes, may be from the descending course being swiftest in the middle, or centre of the spout; this naturally drawing the outer parts inward, and the centre to a point; and that will appear foremost that moves swiftest. The phenomenon of retiring and advancing, I think may be accounted for, by supposing the progressive motion to exceed or not equal the consumption of the vapor by condensation. Or more plainly thus; the descending vapor which forms the apparent spout, if it be slow in its progress downwards, is condensed as fast as it advances, and so appears at a stand; when it is condensed faster than it advances, it appears to retire; and *vice versâ*.

Its duration and manner of ending are as the causes, and may vary by several accidents.

The cloud itself may be so circumstanced as to stop it; as when, extending wide, it weighs down at a distance round about, while a small circle at the spout, being exonerated by the discharge, ascends and shuts up the passage. A new determination of wind may, perhaps, stop it too. Places liable to these appearances are very liable to frequent and sudden alterations of it.

Such accidents as a clap of thunder, firing cannon, &c., may stop them; and the reason may be, that any shock of this kind may occasion the particles, that are near cohering, immediately to do so; and then the

whole, thus condensed, falls at once (which is what I suppose is vulgarly called the breaking of the spout); and in the interval between this period and that of the next set of particles being ready to unite, the spout shuts up. So that, if this reasoning is just, these phenomena agree with my hypothesis.

The usual temper of the air, at the time of their appearance, if I have a right information, is for me too; it being then pretty cool for the season and climate. And this is worth remark, because cool air is weighty, and will not ascend; besides, when the air grows cool, it shows, that the upper region descends, and conveys this temper down; and, when the tempers are equal, no whirlwind can take place. But spouts have been known, when the lower region has been really cold. Gordon's spout in the Downs is an instance of this, (See *Philosophical Transactions*), where the upper region was probably not at all cooler, if so cold as the lower; it was a cold day, in the month of March; hail followed, but not snow; and it is observable, that not so much as hail follows or accompanies them in moderate seasons or climes, when and where they are most frequent. However, it is not improbable, that just about the place of descent may be cooler than the neighbouring parts, and so favor the wonderful celerity of condensation. But, after all, should we allow the under region to be ever so much the hottest, and a whirlwind to take place in it; suppose then the sea-water to ascend, it would certainly cool the spout; and then query, whether it would not very much, if not wholly, obstruct its progress.

It commonly rains when spouts disappear, if it did not before, which it frequently does not, by the best accounts I have had; but the cloud increases much faster after they disappear, and it soon rains. The first shows the spout to be a contracted rain, instead of the diffused one that follows; and the latter, that the cloud

was not formed by ascending water, for then it would have ceased growing when the spout vanished.

However, it seems that spouts have sometimes appeared after it began to rain; but this is one way a proof of my hypothesis, viz. as whirlwinds do not come under a cloud.

I forgot to mention, that the increase of cloud, while the spout subsists, is no argument of an ascent of water by the spout; since thunder-clouds sometimes increase greatly while it rains very hard.

Divers effects of spouts seem not so well accounted for any other way as by descent.

The bush round the feet of them seems to be a great spray of water made by the violence of descent, like that in great falls of water from high precipices.

The great roar, like some vast inland falls, is so different from the roar of whirlwinds, by all accounts, as to be no ways compatible.

The throwing things from it with great force, instead of carrying them up into the air, is another difference.

There seems some probability that the sailors' traditionary belief, that spouts may break in their decks, and so destroy vessels, might originate from some facts of that sort in former times. This danger is apparent on my hypothesis, but it seems not so on the other; and my reason for it is, that the whole column of a spout from the sea to the clouds cannot, in a natural way, even upon the largest supposition, support more than about three feet water, and, from truly supposable causes, not above one foot, as may appear more plainly by and by. Supposing now the largest of these quantities to rise, it must be disseminated into drops, from the surface of the sea to the region of the clouds, or higher; for this reason, it is quite unlikely to be collected into masses, or a body, upon its falling; but

would descend in progression, according to the several degrees of altitude the different portions had arrived at when it received this new determination.

Now that there cannot more rise upon the common hypothesis, than I have mentioned, may appear probable, if we attend to the only efficient cause in supposed ascending spouts, viz. whirlwinds.

We know that the rarefaction of the lower, and the condensation of the upper region of air, are the only natural causes of whirlwinds. Let us, then, suppose the former as hot as their greatest summer heat in England, and the latter as cold as the extent of their winter. These extremes have been found there to alter the weight of the air one tenth, which is equal to a little more than three feet water. Were this case possible, and a whirlwind take place in it, it might act with a force equal to the mentioned difference. But, as this is the whole strength, so much water could not rise; therefore, to allow it due motion upwards, we must abate at least one fourth part, perhaps more, to give it such a swift ascension as some think usual. But here several difficulties occur; at least they are so to me; as, whether this quantity would render the spout opake, since it is plain that in drops it could not do so; how, or by what means, it may be reduced small enough; or, if the water be not reduced into vapor, what will suspend it in the region of the clouds when exonerated there? And, if vaporized while ascending, how can it be dangerous by what they call the breaking? For it is difficult to conceive how a condensative power should instantaneously take place of a rarefying and disseminating one.

The sudden fall of the spout, or rather the sudden ceasing of it, I accounted for, in my way, before. But it seems necessary to mention something I then forgot. Should it be said to do so (that is, to fall), because all

the lower rarefied air is ascended, whence the whirlwind must cease, and its burden drop; I cannot agree to this, unless the air be observed on a sudden to have grown much colder, which I cannot learn has been the case. Or, should it be supposed that the spout was, on a sudden, obstructed at the top, and this the cause of the fall, however plausible this might appear, yet no more water would fall than what was at the same time contained in the column, which is often, by many and satisfactory accounts to me, again far from being the case.

We are, I think, sufficiently assured, that not only tons, but scores of hundreds of tons, descend in one spout; scores of tons more than can be contained in the trunk of it, should we suppose water to ascend.

But, after all, it does not appear, that the abovementioned different degrees of heat and cold concur in any region where spouts usually happen, nor, indeed, in any other.

Observations on the Meteorological Paper; by a Gentleman in Connecticut.[*]

READ AT THE ROYAL SOCIETY, NOVEMBER 4TH, 1756.

"AIR and water mutually attract each other," (saith Mr. Franklin,) "hence water will dissolve in air, as salt in water." I think that he hath demonstrated, that the supporting of salt in water is not owing to its superficies being increased, because "the specific gravity of salt is not altered by dividing of it, any more than that of lead, sixteen bullets of which, of an ounce each,

[*] This gentleman was Mr. Jonathan Todd, who wrote the "Observations" in a letter to Mr. Eliot, by whom they were communicated to Franklin. — EDITOR.

weigh as much in water as one of a pound." But yet, when this came to be applied to the supporting of water in air, I found an objection rising in my mind.

In the first place, I have always been loath to seek for any new hypothesis, or particular law of nature, to account for any thing that may be accounted for from the known, general, and universal law of nature; it being an argument of the infinite wisdom of the Author of the world, to effect so many things by one general law. Now I had thought that the rising and support of water in air, might be accounted for from the general law of gravitation, by only supposing the spaces occupied by the same quantity of water increased.

And with respect to the lead, I queried thus in my own mind; whether, if the superficies of a bullet of lead should be increased four or five fold by an internal vacuity, it would weigh the same in water as before. I mean, if a pound of lead should be formed into a hollow globe, empty within, whose superficies should be four or five times as big as that of the same lead when a solid lump, it would weigh as much in water as before. I supposed it would not. If this concavity was filled with water, perhaps it might; if with air, it would weigh at least as much less, as the difference between the weight of that included air and that of water.

Now, although this would do nothing to account for the dissolution of salt in water, the smallest lumps of salt being no more hollow spheres, or any thing of the like nature, than the greatest; yet, perhaps, it might account for water's rising and being supported in air. For you know that such hollow globules, or bubbles, abound upon the surface of the water, which, even by the breath of our mouths, we can cause to quit the water, and rise in the air.

These bubbles I used to suppose to be coats of water, containing within them air rarefied and expand-

ed with fire, and that, therefore, the more friction and dashing there is upon the surface of the waters, and the more heat and fire, the more they abound.

And I used to think, that, although water be specifically heavier than air, yet such a bubble, filled only with fire and very rarefied air, may be lighter than a quantity of common air, of the same cubical dimensions, and therefore ascend; for the rarefied air enclosed may more fall short of the same bulk of common air in weight, than the watery coat exceeds a like bulk of common air in gravity.

This was the objection in my mind, though, I must confess, I know not how to account for the watery coat's encompassing the air, as above mentioned, without allowing the attraction between air and water, which the gentleman supposes; so that I do not know but that this objection, examined by that sagacious genius, will be an additional confirmation of the hypothesis.

The gentleman observes, "that a certain quantity of moisture should be every moment discharged and taken away from the lungs;" and hence accounts for the suffocating nature of snuffs of candles, as impregnating the air with grease, between which and water there is a natural repellency; and of air that hath been frequently breathed in, which is overloaded with water, and, for that reason, can take no more air. Perhaps the same observation will account for the suffocating nature of damps in wells.

But then, if the air can support and take off but such a proportion of water, and it is necessary that water be so taken off from the lungs, I queried with myself how it is we can breathe in an air full of vapors, so full as that they continually precipitated. Do not we see the air overloaded, and casting forth water plentifully when there is no suffocation?

The gentleman again observes, "that the air under the equator, and between the tropics, being constantly heated and rarefied by the sun, rises; its place is supplied by the air from northern and southern latitudes, which, coming from parts where the air and earth had less motion, and not suddenly acquiring the quicker motion of the equatorial earth, appears an east wind blowing westward; the earth moving from west to east, and slipping under the air."

In reading this, two objections occurred to my mind.

First, that it is said, the trade-wind doth not blow in the forenoon, but only in the afternoon.

Secondly, that either the motion of the northern and southern air towards the equator is so slow, as to acquire almost the same motion as the equatorial air when it arrives there, so that there will be no sensible difference; or else the motion of the northern and southern air towards the equator is quicker, and must be sensible; and then the trade-wind must appear either as a southeast or northeast wind; south of the equator, a southeast wind; north of the equator, a northeast. For the apparent wind must be compounded of this motion from north to south, or *vice versâ*; and of the difference between its motion from west to east, and that of the equatorial air.

Observations in Answer to the Foregoing; by Benjamin Franklin.

READ AT THE ROYAL SOCIETY, NOVEMBER 4TH, 1756.

1. THE supposing a mutual attraction between the particles of water and air is not introducing a new law of nature; such attractions taking place in many other known instances.

2. Water is specifically eight hundred and fifty times heavier than air. To render a bubble of water, then, specifically lighter than air, it seems to me, that it must take up more than eight hundred and fifty times the space it did before it formed the bubble; and within the bubble should be either a vacuum, or air rarefied more than eight hundred and fifty times. If a vacuum, would not the bubble be immediately crushed by the weight of the atmosphere? And no heat, we know of, will rarefy air any thing near so much; much less the common heat of the sun, or that of friction, by the dashing on the surface of the water. Besides, water agitated ever so violently produces no heat, as has been found by accurate experiments.

3. A hollow sphere of lead has a firmness and consistency in it, that a hollow sphere or bubble of fluid, unfrozen water cannot be supposed to have. The lead may support the pressure of the water it is immerged in, but the bubble could not support the pressure of the air, if empty within.

4. Was ever a visible bubble seen to rise in air? I have made many, when a boy, with soap-suds and a tobacco-pipe; but they all descended when loose from the pipe, though slowly, the air impeding their motion. They may, indeed, be forced up by a wind from below, but do not rise of themselves, though filled with warm breath.

5. The objection relating to our breathing moist air seems weighty, and must be farther considered. The air that has been breathed has, doubtless, acquired an addition of the perspirable matter which nature intends to free the body from, and which would be pernicious if retained and returned into the blood; such air, then, may become unfit for respiration, as well for that reason, as on account of its moisture. Yet I should be

glad to learn, by some accurate experiment, whether a draft of air, two or three times inspired and expired, perhaps in a bladder, has, or has not, acquired more moisture than our common air in the dampest weather. As to the precipitation of water in the air we breathe, perhaps it is not always a mark of that air's being overloaded. In the region of the clouds, indeed, the air must be overloaded, if it let fall its water in drops, which we call rain; but those drops may fall through a drier air near the earth; and accordingly we find, that the hygroscope sometimes shows a less degree of moisture during a shower, than at other times when it does not rain at all. The dewy dampness, that settles on the insides of our walls and wainscots, seems more certainly to denote an air overloaded with moisture; and yet this is no sure sign; for, after a long-continued cold season, if the air grows suddenly warm, the walls, &c. continuing longer their coldness, will, for some time, condense the moisture of such air, till they grow equally warm, and then they condense no more, though the air is not become drier. And, on the other hand, after a warm season, if the air grows cold, though moister than before, the dew is not so apt to gather on the walls. A tankard of cold water will, in a hot and dry summer's day, collect a dew on its outside; a tankard of hot water will collect none in the moistest weather.

6. It is, I think, a mistake, that the trade-winds blow only in the afternoon. They blow all day and all night, and all the year round, except in some particular places. The southerly sea-breezes on your coasts, indeed, blow chiefly in the afternoon. In the very long run from the west side of America to Guam, among the Philippine Islands, ships seldom have occasion to hand their sails, so equal and steady is the

gale, and yet they make it in about sixty days, which could not be if the wind blew only in the afternoon.

7. That really is, which the gentleman justly supposes ought to be on my hypothesis. In sailing southward, when you first enter the trade wind, you find it northeast, or thereabouts, and it gradually grows more east as you approach the line. The same observation is made of its changing from southeast to east gradually, as you come from the southern latitudes to the equator.

Observations on the Meteorological Paper; sent by Mr. Cadwallader Colden to B. Franklin.

READ AT THE ROYAL SOCIETY, NOVEMBER 4TH, 1756.

19 November, 1753.

THAT power by which the air expands itself, you attribute to a mutual repelling power in the particles, which compose the air, by which they are separated from each other with some degree of force. Now this force, on this supposition, must not only act when the particles are in mutual contact, but likewise when they are at some distance from each other. How can two bodies, whether they be great or small, act at any distance, whether that distance be small or great, without something intermediate on which they act? For, if any body act on another at any distance from it, however small that distance be, without some medium to continue the action, it must act where it is not, which to me seems absurd.

It seems to me, for the same reason, equally absurd to give a mutual attractive power between any other

particles supposed to be at a distance from each other, without any thing intermediate to continue their mutual action. I can neither attract nor repel any thing at a distance, without something between my hand and that thing, like a string or a stick; nor can I conceive any mutual action without some middle thing, when the action is continued to some distance.

The increase of the surface of any body lessens its weight, both in air and water, or any other fluid, as appears by the slow descent of leaf-gold in the air.

The observation of the different density of the upper and lower air, from heat and cold, is good, and I do not remember it is taken notice of by others; the consequences also are well drawn; but, as to winds, they seem principally to arise from some other cause. Winds generally blow from some large tracts of land, and from mountains. Where I live, on the north side of the mountains, we frequently have a strong southerly wind, when they have as strong a northerly wind, or calm, on the other side of these mountains. The continual passing of vessels on Hudson's River, through these mountains, give frequent opportunities of observing this.

In the spring of the year the sea-wind (by a piercing cold) is always more uneasy to me, accustomed to winds which pass over a tract of land, than the northwest wind.

You have received the common notion of water-spouts, which, from my own ocular observation, I am persuaded is a false conception. In a voyage to the West Indies, I had an opportunity of observing many water-spouts. One of them passed nearer than thirty or forty yards to the vessel I was in, which I viewed with a good deal of attention; and, though it be now forty years since I saw it, it made so strong an impres-

sion on me, that I very distinctly remember it. These water-spouts were in the calm latitudes, that is, between the trade and the variable winds, in the month of July. That spout which passed so near us was an inverted cone, with the *tip* or *apex* towards the sea, and reached within about eight feet of the surface of the sea, its basis in a large black cloud. We were entirely becalmed. It passed slowly by the vessel. I could plainly observe, that a violent stream of wind issued from the spout, which made a hollow of about six feet diameter in the surface of the water, and raised the water in a circular uneven ring round the hollow, in the same manner that a strong blast from a pair of bellows would do, when the pipe is placed perpendicular to the surface of the water; and we plainly heard the same hissing noise, which such a blast of wind must produce on the water. I am very sure there was nothing like the sucking of water from the sea into the spout, unless the spray, which was raised in a ring to a small height, could be mistaken for a raising of water. I could plainly distinguish a distance of about eight feet between the sea and the tip of the cone, in which nothing interrupted the sight, which must have been, had the water been raised from the sea.

In the same voyage I saw several other spouts at a greater distance, but none of them whose tip of the cone came so near the surface of the water. In some of them the axis of the cone was considerably inclined from the perpendicular, but in none of them was there the least appearance of sucking up water. Others of them were bent or arched. I believe, that a stream of wind issued from all of them; and it is from this stream of wind that vessels are often overset, or founder at sea suddenly. I have heard of vessels being overset

when it was perfectly calm the instant before the stream of wind struck them and immediately after they were overset; which could not otherwise be, but by such a stream of wind from a cloud.

That wind is generated in clouds will not admit of a dispute. Now, if such wind be generated within the body of the cloud, and issue in one particular place, while it finds no passage in the other parts of the cloud, I think it may not be difficult to account for all the appearances in water-spouts; and from hence the reason of breaking those spouts by firing a cannon-ball through them, as thereby a horizontal vent is given to the wind. When the wind is spent, which dilated the cloud, or the fermentation ceases, which generates the air and wind, the clouds may descend in a prodigious fall of water or rain. A remarkable intestine motion, like a violent fermentation, is very observable in the cloud from whence the spout issues. No salt-water, I am persuaded, was ever observed to fall from the clouds, which must certainly have happened if sea-water had been raised by a spout.

Answer to the foregoing Observations; by B. Franklin.

READ AT THE ROYAL SOCIETY, NOVEMBER 4TH, 1756.

I AGREE with you, that it seems absurd to suppose that a body can act where it is not. I have no idea of bodies at a distance attracting or repelling one another without the assistance of some medium, though I know not what that medium is, or how it operates. When I speak of attraction or repulsion, I make use of those words for want of others more proper, and intend only to express *effects* which I see, and not

causes of which I am ignorant. When I press a blown bladder between my knees, and find I cannot bring its sides together, but my knees feel a springy matter, pushing them back to a greater distance, or repelling them, I conclude that the air it contains is the cause. And, when I operate on the air, and find I cannot by pressure force its particles into contact, but they still spring back against the pressure, I conceive there must be some medium between its particles that prevents their closing, though I cannot tell what it is. And, if I were acquainted with that medium, and found its particles to approach and recede from each other, according to the pressure they suffered, I should imagine there must be some finer medium between them, by which these operations were performed.

I allow that increase of the surface of a body may occasion it to descend slower in air, water, or any other fluid; but do not conceive, therefore, that it lessens its weight. Where the increased surface is so disposed as that in its falling, a greater quantity of the fluid it sinks in must be moved out of its way, a greater time is required for such removal. Four square feet of sheet-lead sinking in water *broadways*, cannot descend near so fast as it would *edgeways*, yet its weight in the hydrostatic balance, would, I imagine, be the same, whether suspended by the middle or by the corner.

I make no doubt but that ridges of high mountains do often interrupt, stop, reverberate, or turn the winds that blow against them, according to the different degrees of strength of the winds, and angles of incidence. I suppose, too, that the cold upper parts of mountains may condense the warmer air that comes near them, and so, by making it specifically heavier, cause it to descend on one or both sides of the ridge into the

warmer valleys, which will seem a wind blowing from the mountain.

Damp winds, though not colder by the thermometer, give a more uneasy sensation of cold, than dry ones; because (to speak like an electrician) they *conduct* better; that is, are better fitted to convey away the heat from our bodies. The body cannot feel *without* itself; our sensation of cold is not in the air *without* the body, but in those parts of the body, which have been deprived of their heat by the air. My desk and its lock are, I suppose, of the same temperament when they have been long exposed to the same air; but now, if I lay my hand on the wood, it does not seem so cold to me as the lock; because (as I imagine) wood is not so good a conductor, to receive and convey away the heat from my skin and the adjacent flesh, as metal is. Take a piece of wood, of the size and shape of a dollar, between the thumb and finger of one hand, and a dollar, in like manner, with the other hand; place the edges of both, at the same time, in the flame of a candle; and, though the edge of the wooden piece takes flame, and the metal piece does not, yet you will be obliged to drop the latter before the former, it conducting the heat more suddenly to your fingers. Thus we can, without pain, handle glass and China cups filled with hot liquors, as tea, &c., but not silver ones. A silver tea-pot must have a wooden handle. Perhaps it is for the same reason, that woollen garments keep the body warmer than linen ones equally thick; woollen keeping the natural heat in, or, in other words, not conducting it out to air.

In regard to water-spouts, having, in a long letter to a gentleman of the same sentiment with you as to their direction, said all that I have to say in support of my opinion, I need not repeat the arguments therein con-

tained, as I intend to send you a copy of it by some other opportunity, for your perusal. I imagine you will find all the appearances you saw, accounted for by my hypothesis. I thank you for communicating the account of them. At present I would only say, that the opinion of winds being generated in clouds by fermentation is new to me, and I am unacquainted with the facts on which it is founded. I likewise find it difficult to conceive of winds confined in the body of clouds, which I imagine have little more solidity than the fogs on the earth's surface. The objection from the freshness of rain-water is a strong one; but I think I have answered it in the letter above mentioned, to which I must beg leave at present to refer you.

Extracts from Dampier's Voyages, relating to Water-Spouts.

READ AT THE ROYAL SOCIETY, DECEMBER 16TH, 1756.

"A SPOUT is a small ragged piece or part of a cloud, hanging down about a yard seemingly, from the blackest part thereof. Commonly it hangs down sloping from thence, or sometimes appearing with a small bending, or elbow, in the middle. I never saw any hang perpendicularly down. It is small at the lower end, seeming no bigger than one's arm, but still fuller towards the cloud from whence it proceeds.

"When the surface of the sea begins to work, you shall see the water for about one hundred paces in circumference, foam and move gently round, till the whirling motion increases; and then it flies upwards in a pillar, about one hundred paces in compass at the bottom, but gradually lessening upwards, to the smallness

of the spout itself, through which the rising sea-water seems to be conveyed into the clouds. This visibly appears by the clouds increasing in bulk and blackness. Then you shall presently see the cloud drive along, though before it seemed to be without any motion; the spout also keeping the same course with the cloud, and still sucking up the water as it goes along; and they make a wind as they go. Thus it continues for half an hour, more or less, until the sucking is spent, and then, breaking off, all the water which was below the spout, or pendulous piece of cloud, falls down again into the sea, making a great noise with its falling and clashing motion in the sea.

"It is very dangerous for a ship to be under a spout when it breaks; therefore we always endeavour to shun it, by keeping at a distance, if possibly we can. But, for want of wind to carry us away, we are often in great fear and danger; for it is usually calm when spouts are at work, except only just where they are. Therefore men at sea, when they see a spout coming, and know not how to avoid it, do sometimes fire shot out of their great guns into it, to give it air or vent, that so it may break; but I did never hear that it proved to be of any benefit.

"And, now we are on this subject, I think it not amiss to give you an account of an accident that happened to a ship once on the coast of Guinea, some time in or about the year 1674. One Captain Records, of London, bound for the coast of Guinea, in a ship of three hundred tons and sixteen guns, called *The Blessing*, when he came into latitude seven or eight degrees north, he saw several spouts, one of which came directly towards the ship, and he, having no wind to get out of the way of the spout, made ready to receive it by furling the sails. It came on very swift, and broke a

little before it reached the ship, making a great noise, and raising the sea round it, as if a great house, or some such thing, had been cast into the sea. The fury of the wind still lasted, and took the ship on the starboard bow with such violence, that it snapt off the bowsprit and fore-mast both at once, and blew the ship all along, ready to overset it; but the ship did presently right again, and the wind whirling round took the ship a second time with the like fury as before, but on the contrary side, and was again like to overset her the other way; the mizzen-mast felt the fury of this second blast, and was snapt short off, as the fore-mast and bowsprit had been before. The main-mast and the main-top-mast received no damage, for the fury of the wind (which was presently over) did not reach them. Three men were in the fore-top when the fore-mast broke, and one on the bowsprit, and fell with them into the sea, but all of them were saved. I had this relation from Mr. John Canby, who was then quartermaster and steward of her; one Abraham Wise was chief mate, and Leonard Jefferies second mate.

"We are usually much afraid of them, yet this was the only damage that I ever heard done by them. They seem terrible enough, the rather because they come upon you while you lie becalmed, like a log in the sea, and cannot get out of their way. But, though I have seen and been beset by them often, yet the fright was always the greatest of the harm." — *Dampier*, Vol. I. p. 451.

Account of a Spout on the Coast of New Guinea.

"We had fair, clear weather, and a fine moderate gale from southeast to east-by-north; but at daybreak

the clouds began to fly, and it lightened very much in the east-northeast. At sunrising the sky looked very red in the east near the horizon; and there were many black clouds both to the south and north of it. About a quarter of an hour after the sun was up, there was a squall to the windward of us, when, on a sudden, one of our men on the forecastle called out, that he saw something astern, but could not tell what. I looked out for it, and immediately saw a spout beginning to work within a quarter of a mile of us, exactly in the wind; we presently put right before it. It came very swiftly, whirling the water up in a pillar, about six or seven yards high. As yet I could not see any pendulous cloud from whence it might come; and was in hopes it would soon lose its force. In four or five minutes' time, it came within a cable's length of us, and passed away to leeward; and then I saw a long pale stream coming down to the whirling water. This stream was about the bigness of a rainbow. The upper end seemed vastly high, not descending from any dark cloud, and therefore the more strange to me, I never having seen the like before. It passed about a mile to the leeward of us, and then broke. This was but a small spout, and not strong nor lasting;* yet I perceived much wind in it, as it passed by." — Vol. III. p. 223.

Account of another Spout.

"We saw a spout but a small distance from us. It fell down out of a black cloud that yielded great store

* Probably if it had been lasting, a cloud would have been formed above it. These extracts from Dampier seem, in different instances, to favor both opinions, and, therefore, are inserted entire, for the reader's consideration.

of rain, thunder, and lightning. This cloud hovered to the southward of us for the space of three hours, and then drew to the westward a great pace, at which time it was that we saw the spout, which hung fast to the cloud till it broke, and then the cloud whirled about to the southeast, then to the northeast, where, meeting with an island, it spent itself, and so dispersed; and immediately we had a little of the tail of it, having had none before." — Vol. III. p. 182.

FROM WILLIAM SHERVINGTON TO B. FRANKLIN.

On the Transit of Mercury across the Sun, May 6th, 1753, observed at Antigua.

Antigua, 20 June, 1753.

SIR,

Mr. Benjaman Mecom † having received half a dozen circulating letters from you relating to Mercury's transit over the sun the 6th of last May, he put them into my hands. One would have sufficed for our island, as we are not overburdened with men, who have a taste that way. I send you the result of my observation thereof.

Sunday, May 6th, at 6h. 7m. 51s., I observed the western limb of Mercury to touch the western limb of the Sun; and at 6h. 10m. 37s. he touched the same with his eastern limb, and totally disappeared. Latitude of the place 17° north; longitude, by estimation, 61° 45′ west from London.

This was taken by a Grahame's watch, and corrected

† A nephew of Dr. Franklin's, who was established as a printer in Antigua. — EDITOR.

by two altitudes taken by a most exquisite quadrant. Viz. at 6h. 58m. 7s. I observed the distance of the Sun's upper limb from the zenith = 72° 21′ 30″; and at 9h. 31m. 5s. I observed the same = 36° 17′.

By the common process, I found the watch was 4m. 4s. too fast; therefore,

	from	6h.	10m. 37s.
	take		4m. 4s.
True exact time of Mercury's exit here,		6h.	6m. 33s.

Pray impart your observation to your well-wisher,

WILLIAM SHERVINGTON.

TO THOMAS CLAP.*

On Smeaton's Air-Pump.

Philadelphia, 8 November, 1753.

DEAR SIR,

The first intimation I find of the new air-pump is in a piece of Mr. Watson's read to the Royal Society, February 20th, 1752, where, describing some experiments he made *in vacuo*, he says; "The more complete the vacuum, *cæteris paribus*, the more considerable were the effects; and here I should not do justice to real merit, were I silent in regard to Mr. Smeaton. This gentleman, with a genius truly mechanical, which enables him to give to such philosophical instruments, as he executes, a degree of perfection scarce to be found elsewhere; this gentleman, I say, has construct-

* President of Yale College; eminent as a theologian, and for his attainments in mathematics, astronomy, and the various branches of natural philosophy. — EDITOR.

ed an air-pump, by which we are empowered to make Boyle's vacuum much more perfect than heretofore. By a well conducted experiment, which admits of no doubt as to its truth, I have seen by this pump the air rarefied to one thousand times its natural state; whereas, commonly, we seldom arrive at above one hundred and fifty. As the promotion of the mechanic arts is a considerable object of our excellent institution, if this gentleman could be prevailed upon to communicate to the Royal Society that particular construction of his air-pump, which enables it to execute so much more than those commonly in use, it would not fail to be an acceptable present."

So far Mr. Watson. In April following, was read a letter from Mr. Smeaton, in which he describes his improvement, and gives a draft of his pump; the whole too long to transcribe; but it appears to me, that the machine, being rather simplified than made more complex, can scarce cost more than one of the old sort, though the price is not mentioned. By only turning a cock it is at pleasure made a condensing engine; an advantage the others have not.

I have seen nothing of your searchers. Mr. Parker has received Bower, but writes me, that he is at a loss how to send it, and desires you would order somebody to call for it.

I shall send the dollars for Mr. Mix per next post; for I fancy you will not now buy this apparatus here, but choose the new air-pump from England.

With my respects to all friends, I am,

Dear Sir,

Your obliged humble servant,

B. FRANKLIN.

FROM JAMES BOWDOIN TO B. FRANKLIN.

Concerning the Light in Sea-Water.

READ AT THE ROYAL SOCIETY, DECEMBER 6TH, 1756.

12 November, 1753.

WHEN I was at the eastward, I had an opportunity of observing the luminous appearance of the sea when disturbed; at the head and stern of the vessel, when under way, it appeared very bright. The best opportunity I had to observe it was in a boat, in company with several gentlemen going from Portsmouth, about three miles, to our vessel lying at the mouth of Piscataqua River. Soon after we set off (it being in the evening) we observed a luminous appearance, where the oars dashed the water. Sometimes it was very bright, and afterwards, as we rowed along, gradually lessened, till almost imperceptible, and then re-illumined. This we took notice of several times in the passage. When I got on board the vessel, I ordered a pail to be dipped up, full of sea-water, in which, on the water's being moved, a sparkling light appeared. I took a linen cloth, and strained some of the water through it, and there was a like appearance on the cloth, which soon went off; but on rubbing the cloth with my finger, it was renewed. I then carried the cloth to the light, but could not perceive any thing upon it which should cause that appearance.

Several gentlemen were of opinion, that the separated particles of putrid animal and other bodies, floating on the surface of the sea, might cause that appearance; for putrid fish, &c., they said, will cause it; and the sea animals which have died, and other bodies putrefied therein since the creation, might afford a

sufficient quantity of these particles to cover a considerable portion of the surface of the sea; which particles, being differently dispersed, might account for the different degrees of light in the appearance above mentioned. But this account seems liable to this obvious objection, that as putrid fish, &c. make a luminous appearance without being moved or disturbed, it might be expected that the supposed putrid particles on the surface of the sea should always appear luminous, where there is not a greater light; and, consequently, that the whole surface of the sea, covered with those particles, should always in dark nights, appear luminous, without being disturbed. But this is not fact.

Among the rest, I threw out my conjecture, that the said appearance might be caused by a great number of little animals, floating on the surface of the sea, which, on being disturbed, might, by expanding their fins, or otherwise moving themselves, expose such a part of their bodies as exhibits a luminous appearance, somewhat in the manner of a glow-worm or fire-fly that these animals may be more numerous in some places than others; and, therefore, that the appearance above mentioned, being fainter and stronger in different places, might be owing to that; that certain circumstances of weather, &c. might invite them to the surface, on which in a calm they might sport themselves and glow; or in storms, being forced up, make the same appearance.

There is no difficulty in conceiving that the sea may be stocked with animalcula for this purpose, as we find all nature crowded with life. But it seems difficult to conceive that such small portions of matter, even if they were wholly luminous, should affect our sight; much more so, when it is supposed that only a part of them is luminous. But, if we consider some other

appearances, we may find the same difficulty to conceive of them; and yet we know they take place. For instance, the flame of a candle, which, it is said, may be seen four miles round. The light which fills this circle of eight miles diameter was contained, when it first left the candle, within a circle of half an inch diameter. If the density of light, in these circumstances, be as those circles to each other, that is, as the squares of their diameters, the candle-light, when come to the eye, will be 1,027,709,337,600 times rarer than when it quitted the half-inch circle. Now the aperture of the eye, through which the light passes, does not exceed one tenth of an inch diameter, and the portion of the lesser circle, which corresponds to this small portion of the greater circle, must be proportionably, that is, 1,027,709,337,600 less than one tenth of an inch; and yet this infinitely small point (if you will allow the expression) affords light enough to make it visible four miles; or, rather, affords light sufficient to affect the sight at that distance.

The smallness of the animalcula is no objection, then, to this conjecture; for, supposing them to be ten thousand times less than the *minimum visibile*, they may, notwithstanding, emit light enough to affect the eyes, and so to cause the luminous appearance aforesaid. This conjecture I send you for want of something better.

I am, with the greatest esteem, &c.

JAMES BOWDOIN.*

* See Dr. Franklin's remarks on this letter, in his answer, dated December 13th, 1753, in Vol. V. p. 337. — EDITOR.

FROM M. DALIBARD TO B. FRANKLIN.

TRANSLATION.

Intelligence on Philosophical Subjects.

Paris, 31 March, 1754.

MY DEAR FRIEND,

I received on the 15th of January last, your obliging letter of October 28th. I did not reply to it immediately, because I thought it best to wait for what you were to send me by the next vessel, which you informed me would sail eight or ten days later. I have waited till this time, without having received any thing but a small parcel of seeds, and a very kind letter from our respected friend, Mr. Bartram. Our worthy correspondent, Mr. Collinson, forwarded it to me by a mutual friend, who furnishes me with another opportunity of sending letters to England. It is only two days since I received Mr. Bartram's packet, and I beg you will have the kindness to send him my answer.

I learned, a short time before I received your last letter, that the vessel, on board which was the parcel of books which I sent to Mr. Collinson for you, had been lost at the mouth of the Thames. I regretted extremely this loss, which it is not possible for me fully to repair, as there were some books among them, which I had received from Germany, and which I cannot obtain again here. I intend to send you, by the first opportunity I can find direct to London, the books for which you asked me, namely, the four volumes of Buffon's *Natural History*, two copies of your *Letters*, and the *Maps of North and South America* by Delisle and Buache. To these I shall add two copies of my *Flora Parisiensis;* and if the translation, which I am causing to be made, of a work on Electricity by M. Beccaria,

presented to me by the author, is finished, I shall send you the original Italian.

The hope of soon receiving what you announced to me in your letter of October 28th, has caused me to delay the new edition of your *Letters on Electricity* till this time. Besides this, urgent business of various kinds has so occupied my time during the whole of the last winter, that I have been able to devote but few moments to philosophical studies. I have regretted, and still regret this the more, as we have had in this country the finest weather for electrical experiments. I have not been able to procure a sulphur globe, such as I proposed to have, because I have not had time to be present while it was making. It is owing to this same want of time, that the attempt to make my electrometer has not yet proved successful. But I have not given up the one or the other. I hope to give my attention to them shortly, as soon as I shall be somewhat relieved from the pressure of business; and I shall again devote myself to electricity, which still seems to me worthy of the most diligent study.

All our philosophical friends, Messrs. Buffon, Fonferrière, Marty, &c., charge me to make you their best compliments, and M. Dubourg also. We are all waiting with the greatest eagerness to hear from you. I beg that you will let me have letters as soon and often as possible. Your name is venerated in this country as it deserves to be. There are but few electricians, like the Abbé Nollet, whose jealousy is excited by the honor your discoveries have obtained.

With great respect and esteem,

I am, &c.

DALIBARD.

FROM CADWALLADER COLDEN TO B. FRANKLIN.

Water-Spouts. — Wind generated by Fermentation. — Winds blowing in contrary Directions.

READ AT THE ROYAL SOCIETY, DECEMBER 6TH, 1756.

2 April, 1754.

ANY knowledge I have of the winds, and other changes which happen in the atmosphere, is so very defective, that it does not deserve the name; neither have I received any satisfaction from the attempts of others on this subject. It deserves then your thoughts, as a subject in which you may distinguish yourself, and be useful.

Your notion of some things conducting heat or cold better than others pleases me, and I wish you may pursue the scent. If I remember right, Dr. Boerhaave, in his Chemistry, thinks that heat is propagated by the vibration of a subtile, elastic fluid, dispersed through the atmosphere and through all bodies. Sir Isaac Newton says, there are many phenomena to prove the existence of such a fluid; and this opinion has my assent to it. I shall only observe, that it is essentially different from that which I call ether; for ether, properly speaking, is neither a fluid nor elastic; its power consists in reacting any action communicated to it, with the same force it receives the action.

I long to see your explication of water-spouts; but I must tell you beforehand, that it will not be easy for you to convince me, that the principal phenomena were not occasioned by a stream of wind issuing with great force; my eyes and ears both concurring to give me this sentiment, I could have no more evidence than to feel the effects, which I had no inclination to do.

It surprises me a little, that wind, generated by fermentation, is new to you, since it may be every day observed in fermenting liquor. You know with what force fermenting liquors will burst the vessels which contain them, if the generated wind have not vent; and with what force it issues on giving it a small vent, or by drawing the cork of a bottle. Dr. Boerhaave says, that the steam issuing from fermenting liquors received through a very small vent-hole, into the nose, will kill as suddenly and certainly as lightning. That air is generated by fermentation, I think you will find fully proved in Dr. Hale's *Analysis of the Air*, in his "Vegetable Statics." If you have not read the book, you have a new pleasure to come.

The solution you give to the objection I made from the contrary winds blowing from the opposite sides of the mountains, from their being eddies, does not please me, because the extent of these winds is by far too large to be occasioned by any eddy. It is forty miles from New York to our mountains, through which Hudson's River passes. The river runs twelve miles in the mountains, and from the north side of the mountains it is about ninety miles to Albany. I have myself been on board a vessel more than once, when we have had a strong northerly wind against us, all the way from New York, for two or three days. We have met vessels from Albany, who assured us that, on the other side of the mountains, they had, at the same time, a strong, continued southerly wind against them; and this frequently happens.

I have frequently seen, both on the river, in places where there could be no eddy-winds, and on the open sea, two vessels sailing with contrary winds, within half a mile of each other; but this happens only in easy winds, and generally calm in other places near these winds.

You have, no doubt, frequently observed a single cloud pass, from which a violent gust of wind issues, but of no great extent. I have observed such a gust make a lane through the woods, of some miles in length, by laying the trees flat to the ground, and not above eight or ten chains in breadth. Though the violence of the wind be in the same direction in which the cloud moves and precedes it, yet wind issues from all sides of it; so that, supposing the cloud moves southeasterly, those on the northeast side of it feel a southwest wind, and others on the southwest side, a northeast. And where the cloud passes over, we frequently have a southeast wind from the hinder part of it, but none violent, except the wind in the direction in which the cloud moves. To show what it is which prevents the wind from issuing out equally on all sides, is not an easy problem to me, and I shall not attempt to solve it; but when you shall show what it is which restrains the electrical fluid from spreading itself into the air surrounding it, when it rushes with great violence through the air along, or in the conductor, for a great extent in length, then I may hope to explain the other problem, and remove the difficulty we have in conceiving it.

FROM CADWALLADER COLDEN TO B. FRANKLIN.

New Theory of the Motion of the Planets best suited for calculating Astronomical Tables. — Abbé Nollet's Book.

Coldenham, 2 April, 1754.

DEAR SIR,

I should have acknowledged your favor of the 1st of January sooner, if you had not at the same time told

me, that you were to be from home for some time after the writing of it; and I had my mind engaged in a chain of thinking that I was unwilling to interrupt, as that season was the only time of the year in which I could hope to pursue it without interruption. My design was to show, by the best observations made in the space of two hundred and seventy years, that the theory of the motion of the planets, which I have formed, agrees better with the appearances in the heavens than any theory hitherto published; and that tables may be formed on that theory more correct than any that have hitherto appeared, without excepting those formed by Dr. Halley and published so lately as 1749, which I can show, from Mr. Flamsteed's and Lord Maclesfield's observations, to be almost everywhere faulty. This I am the more fond of, because it is not so much for the pleasure of imagination and amusement, as of real use, but attended with exceeding great care, attention, and trouble.

You have given me an anxious longing to see your answer to the Abbé Nollet's objections to your theory, as I make no doubt of its being made more evident thereby, and cleared from any obscurities, which such things are unavoidably subject to, at first appearing, especially with those that are not much conversant with the subject. Persons who write in the manner the Abbé does, not for the sake of truth and information, but to secure or gain a reputation, seldom avoid losing what they chiefly intend to preserve. The justice, which I hear is done to your merit by the Royal Society, makes full recompense for these little abuses, and I heartily congratulate you upon it.

If you think proper to communicate your answer to the Abbé Nollet to me, before you transmit it, you may assure yourself of my making remarks with that free-

dom which should always subsist between friends, and with less care of exposing my own want of knowledge, than of observing on every thing that I shall think may want correction.

I am, &c.

CADWALLADER COLDEN.

FROM CADWALLADER COLDEN TO P. COLLINSON.

On Water-Spouts.

Coldenham, 28 May, 1754.

DEAR SIR,

Your favor of the 10th of March surprised me, by finding that you had a copy of a letter, which I wrote last fall, or beginning of winter, to Mr. Franklin on water-spouts; because I did not write it with any thoughts, that it would have gone farther than for his private perusal; but, since you take notice of it as a novelty, which destroys the commonly received opinion, you lay me under some necessity of adding something more to it. For, as to the facts therein related, they are certainly true as far as I can trust my eyes and ears. I did, at the time, commit the account of what I saw to writing; but it is so long since, that these, with several other observations made about that time, are lost, or perhaps some one of my friends got the perusal of them, and has not restored them. But the appearance made so strong an impression on me, that I dare trust to my memory, even at this distance of time; and perhaps few, who were curious enough to observe this phenomenon, without being prejudiced by received opinions, if any have had an opportunity of

viewing a water-spout so near them, as I did this, without receiving some injury from it; and indeed the master and sailors were more anxious from the danger of its approach, in preparing against the danger, than in observing it accurately as I did.

I saw several others in the same voyage, at a greater distance, and, from all of them, I am as fully convinced as I can be of any thing from my senses, that the common opinion of sucking up the sea-water is false. If the water had been sucked up, there must have been a rising in the sea directly under the point of the spout; whereas there was a hollow in the sea, which I distinctly saw, and the spray rising round the hollow as happens by the force of wind from the pipe of a pair of bellows. The avoiding of the danger by firing a shot through the pipe, which you mention as an objection, confirms rather than weakens what I say, by giving a horizontal vent to the wind, by which it goes over the ship, and does not strike.

If you consider the particulars in the description of the spouts, which I saw, as I wrote them to Mr. Franklin in that and in a subsequent letter, you will be convinced that the spout was not formed by sucking water. I would not have it thought that, in my opinion, never any great quantity of rain or water falls from those clouds. On the contrary, I think, that, after the fermentation in the clouds, which supports them in the air and generates the wind, is lessened or ceases, then they may fall in prodigious pouring showers, but not confined to the diameter of two or three yards, as the wind from the spout is. The rain, I believe, never or seldom falls at the time the spout is seen, but afterwards and at distances from that place. If the clouds sucked up the sea-water, when it fell again it must at least be brackish. I cannot conceive how the

sucking up of sea-water can divest it of its saline qualities; or if the water were divested of its salt in the clouds, the salt must fall down again somewhere to the sea or land. Salt water, I think, was never observed to come from the clouds.

Mr. Franklin tells me, that another gentleman was of the same opinion with me, of wind issuing from the water-spout, though neither of us knew the other's opinion, and I am still ignorant of the facts on which he formed his.

I am, &c.

CADWALLADER COLDEN.

TO PETER COLLINSON.

Account of a Whirlwind in Maryland.

Philadelphia, 25 August, 1755.

DEAR SIR,

As you have my former papers on whirlwinds, &c., I now send you an account of one which I had lately an opportunity of seeing and examining myself.

Being in Maryland, riding with Colonel Tasker, and some other gentlemen, to his country-seat, where I and my son were entertained by that amiable and worthy man with great hospitality and kindness, we saw, in the vale below us, a small whirlwind beginning in the road, and showing itself by the dust it raised and contained. It appeared in the form of a sugar-loaf, spinning on its point, moving up the hill towards us, and enlarging as it came forward. When it passed by us, its smaller part near the ground appeared no bigger than a common barrel; but, widening upwards,

it seemed, at forty or fifty feet high, to be twenty or thirty feet in diameter. The rest of the company stood looking after it; but, my curiosity being stronger, I followed it, riding close by its side, and observed its licking up, in its progress all the dust that was under its smaller part. As it is a common opinion that a shot, fired through a water-spout, will break it, I tried to break this little whirlwind, by striking my whip frequently through it, but without any effect. Soon after, it quitted the road and took into the woods, growing every moment larger and stronger, raising, instead of dust, the old dry leaves with which the ground was thick covered, and making a great noise with them and the branches of the trees, bending some tall trees round in a circle swiftly and very surprisingly, though the progressive motion of the whirl was not so swift but that a man on foot might have kept pace with it; but the circular motion was amazingly rapid. By the leaves it was now filled with, I could plainly perceive, that the current of air they were driven by, moved upwards in a spiral line; and when I saw the passing whirl continue entire, after leaving the trunks and bodies of large trees which it had enveloped, I no longer wondered that my whip had no effect on it in its smaller state. I accompanied it about three quarters of a mile, till some limbs of dead trees, broken off by the whirl, flying about and falling near me, made me more apprehensive of danger; and then I stopped, looking at the top of it as it went on, which was visible, by means of the leaves contained in it, for a very great height above the trees. Many of the leaves, as they got loose from the upper and widest part, were scattered in the wind; but so great was their height in the air, that they appeared no bigger than flies. My son, who was by this time come up with me, followed the whirlwind till it left the woods,

and crossed an old tobacco-field, where, finding neither dust nor leaves to take up, it gradually became invisible below, as it went away over that field. The course of the general wind then blowing was along with us as we travelled, and the progressive motion of the whirlwind was in a direction nearly opposite, though it did not keep a straight line, nor was its progressive motion uniform, it making little sallies on either hand as it went, proceeding sometimes faster and sometimes slower, and seeming sometimes for a few seconds almost stationary, then starting forward pretty fast again. When we rejoined the company, they were admiring the vast height of the leaves now brought by the common wind over our heads. These leaves accompanied us as we travelled, some falling now and then round about us, and some not reaching the ground till we had gone near three miles from the place where we first saw the whirlwind begin. Upon my asking Colonel Tasker if such whirlwinds were common in Maryland, he answered pleasantly, "No, not at all common; but we got this on purpose to treat Mr. Franklin." And a very high treat it was to,

Dear Sir,

Your affectionate friend and humble servant,

B. FRANKLIN.

TO JOHN LINING, AT CHARLESTON, SOUTH CAROLINA.

On Cold produced by Evaporation.

New York, 14 April, 1757.

SIR,

It is a long time since I had the pleasure of a line from you; and, indeed, the troubles of our country,

with the hurry of business I have been engaged in on that account, have made me so bad a correspondent, that I ought not to expect punctuality in others.

But, being about to embark for England, I could not quit the continent without paying my respects to you, and, at the same time, taking leave to introduce to your acquaintance a gentleman of learning and merit, Colonel Henry Bouquet, who does me the favor to present you this letter, and with whom I am sure you will be much pleased.

Professor Simson, of Glasgow, lately communicated to me some curious experiments of a physician of his acquaintance, by which it appeared, that an extraordinary degree of cold, even to freezing, might be produced by evaporation. I have not had leisure to repeat and examine more than the first and easiest of them, viz. Wet the ball of a thermometer by a feather dipped in spirit of wine, which has been kept in the same room, and has, of course, the same degree of heat or cold. The mercury sinks presently three or four degrees, and the quicker, if, during the evaporation, you blow on the ball with bellows; a second wetting and blowing, when the mercury is down, carries it yet lower. I think I did not get it lower than five or six degrees from where it naturally stood, which was, at that time, sixty. But it is said, that a vessel of water being placed in another somewhat larger, containing spirit, in such a manner that the vessel of water is surrounded with the spirit, and both placed under the receiver of an air-pump; on exhausting the air, the spirit, evaporating, leaves such a degree of cold as to freeze the water, though the thermometer, in the open air, stands many degrees above the freezing point.

I know not how this phenomenon is to be accounted for; but it gives me occasion to mention some loose

notions relating to heat and cold, which I have for some time entertained, but not yet reduced into any form. Allowing common fire, as well as electrical, to be a fluid capable of permeating other bodies, and seeking an equilibrium, I imagine some bodies are better fitted by nature to be conductors of that fluid than others; and that, generally, those which are the best conductors of the electrical fluid, are also the best conductors of this; and *e contra.*

Thus a body which is a good conductor of fire readily receives it into its substance, and conducts it through the whole to all the parts, as metals and water do; and if two bodies, both good conductors, one heated, the other in its common state, are brought into contact with each other, the body which has most fire readily communicates of it to that which had least, and that which had least readily receives it, till an equilibrium is produced. Thus, if you take a dollar between your fingers with one hand, and a piece of wood, of the same dimensions, with the other, and bring both at the same time to the flame of a candle, you will find yourself obliged to drop the dollar before you drop the wood, because it conducts the heat of the candle sooner to your flesh. Thus, if a silver tea-pot had a handle of the same metal, it would conduct the heat from the water to the hand, and become too hot to be used; we therefore give to a metal tea-pot a handle of wood, which is not so good a conductor as metal. But a china or stone tea-pot being in some degree of the nature of glass, which is not a good conductor of heat, may have a handle of the same stuff. Thus, also, a damp moist air shall make a man more sensible of cold, or chill him more, than a dry air that is colder, because a moist air is fitter to receive and conduct away the heat of his body. This fluid, entering bodies

in great quantity first expands them by separating their parts a little, afterwards, by farther separating their parts, it renders solids fluid, and at length dissipates their parts in air. Take this fluid from melted lead, or from water, the parts cohere again; the first grows solid, the latter becomes ice; and this is sooner done by the means of good conductors. Thus, if you take, as I have done, a square bar of lead, four inches long, and one inch thick, together with three pieces of wood

planed to the same dimensions, and lay them, as in the margin, on a smooth board, fixed so as not to be easily separated or moved, and pour into the cavity they form, as much melted lead as will fill it, you will see the melted lead chill, and become firm, on the side next the leaden bar, some time before it chills on the other three sides in contact with the wooden bars, though, before the lead was poured in, they might all be supposed to have the same degree of heat or coldness, as they had been exposed in the same room to the same air. You will likewise observe, that the leaden bar, as it has cooled the melted lead more than the wooden bars have done, so it is itself more heated by the melted lead. There is a certain quantity of this fluid, called fire, in every living human body, which fluid, being in due proportion, keeps the parts of the flesh and blood at such a just distance from each other, as that the flesh and nerves are supple, and the blood fit for circulation. If part of this due proportion of fire be conducted away, by means of a contact with other bodies, as air, water, or metals, the parts of our skin and flesh that come into such contact first draw more near together than is agreeable, and give that sensation which we call cold; and

if too much be conveyed away, the body stiffens, the blood ceases to flow, and death ensues. On the other hand, if too much of this fluid be communicated to the flesh, the parts are separated too far, and pain ensues, as when they are separated by a pin or lancet. The sensation, that the separation by fire occasions, we call heat, or burning. My desk on which I now write, and the lock of my desk, are both exposed to the same temperature of the air, and have therefore the same degree of heat or cold; yet if I lay my hand successively on the wood and on the metal, the latter feels much the coldest, not that it is really so, but, being a better conductor, it more readily than the wood takes away and draws into itself the fire that was in my skin. Accordingly if I lay one hand, part on the lock, and part on the wood, and, after it has lain so some time, I feel both parts with my other hand, I find the part that has been in contact with the lock, very sensibly colder to the touch, than the part that lay on the wood. How a living animal obtains its quantity of this fluid, called fire, is a curious question. I have shown, that some bodies (as metals) have a power of attracting it stronger than others; and I have sometimes suspected, that a living body had some power of attracting out of the air, or other bodies, the heat it wanted. Thus metals hammered, or repeatedly bent, grow hot in the bent or hammered part. But when I consider that air, in contact with the body, cools it; that the surrounding air is rather heated by its contact with the body; that every breath of cooler air, drawn in, carries off part of the body's heat when it passes out again; that therefore there must be in the body a fund for producing it, or otherwise the animal would soon grow cold; I have been rather inclined to think, that the fluid *fire*, as well as the fluid *air*, is attracted

by plants in their growth, and becomes consolidated with the other materials of which they are formed, and makes a great part of their substance; that, when they come to be digested, and to suffer in the vessels a kind of fermentation, part of the fire, as well as part of the air, recovers its fluid, active state again, and diffuses itself in the body digesting and separating it; that the fire, so reproduced by digestion and separation, continually leaving the body, its place is supplied by fresh quantities, arising from the continual separation; that whatever quickens the motion of the fluids in an animal quickens the separation, and reproduces more of the fire, as exercise; that all the fire emitted by wood, and other combustibles, when burning, existed in them before, in a solid state, being only discovered when separating; that some fossils, as sulphur, sea-coal, &c., contain a great deal of solid fire; and that, in short, what escapes and is dissipated in the burning of bodies, besides water and earth, is generally the air and fire that before made parts of the solid. Thus I imagine, that animal heat arises by or from a kind of fermentation in the juices of the body, in the same manner as heat arises in the liquors preparing for distillation, wherein there is a separation of the spirituous, from the watery and earthy parts. And it is remarkable, that the liquor in a distiller's vat, when in its highest and best state of fermentation, as I have been informed, has the same degree of heat with the human body, that is, about 94 or 96.

Thus, as by a constant supply of fuel in a chimney, you keep a warm room, so, by a constant supply of food in the stomach, you keep a warm body; only, where little exercise is used, the heat may possibly be conducted away too fast; in which case such materials are to be used for clothing and bedding, against the

effects of an immediate contact of the air, as are, in themselves, bad conductors of heat, and, consequently, prevent its being communicated through their substance to the air. Hence what is called *warmth* in wool, and its preference, on that account, to linen; wool not being so good a conductor; and hence all the natural coverings of animals, to keep them warm, are such as retain and confine the natural heat in the body, by being bad conductors, such as wool, hair, feathers, and the silk by which the silk-worm, in its tender embryo state, is first clothed. Clothing, thus considered, does not make a man warm by *giving* warmth, but by *preventing* the too quick dissipation of the heat produced in his body, and so occasioning an accumulation.

There is another curious question I will just venture to touch upon, viz. Whence arises the sudden extraordinary degree of cold, perceptible on mixing some chemical liquors, and even on mixing salt and snow, where the composition appears colder than the coldest of the ingredients? I have never seen the chemical mixtures made; but salt and snow I have often mixed myself, and am fully satisfied that the composition feels much colder to the touch, and lowers the mercury in the thermometer more, than either ingredient would do separately. I suppose, with others, that cold is nothing more than the absence of heat or fire. Now, if the quantity of fire before contained or diffused in the snow and salt was expelled in the uniting of the two matters, it must be driven away either through the air or the vessel containing them. If it is driven off through the air, it must warm the air; and a thermometer held over the mixture, without touching it, would discover the heat, by the rising of the mercury, as it must, and always does, in warm air.

This, indeed, I have not tried, but I should guess it

would rather be driven off through the vessel, especially if the vessel be metal, as being a better conductor than air; and so one should find the basin warmer after such mixture. But, on the contrary, the vessel grows cold, and even water, in which the vessel is sometimes placed for the experiment, freezes into hard ice on the basin. Now I know not how to account for this, otherwise than by supposing, that the composition is a better conductor of fire than the ingredients separately, and, like the lock compared with the wood, has a stronger power of attracting fire, and does accordingly attract it suddenly from the fingers, or a thermometer put into it, from the basin that contains it, and from the water in contact with the outside of the basin; so that the fingers have the sensation of extreme cold, by being deprived of much of their natural fire; the thermometer sinks, by having part of its fire drawn out of the mercury; the basin grows colder to the touch, as, by having its fire drawn into the mixture, it is become more capable of drawing and receiving it from the hand; and, through the basin, the water loses its fire that kept it fluid; so it becomes ice. One would expect, that from all this attracted acquisition of fire to the composition, it should become warmer; and, in fact, the snow and salt dissolve at the same time into water, without freezing.

I am, Sir, &c.

B FRANKLIN.

FROM JOHN BARTRAM TO B. FRANKLIN.

Relating to a remarkable Aurora Borealis.

READ AT THE ROYAL SOCIETY, FEBRUARY 25TH, 1762.

Philadelphia, 12 November, 1757.

HERE is a visible Aurora Borealis; at seven o'clock, it was about two hours high, to the northward pretty bright. Soon after daylight disappeared, it was much more east, where it was redder, with some faint streamers, whose points reached near forty-five degrees elevation, which soon disappeared, and the light descended, by degrees, under the pole, and by ten o'clock was near extinct. I should be very glad to know, whether (and how) it appeared this night with you, which may assist in some philosophical inquiries. I have not observed any, this fall, before. Yesterday the weather was cool and clear, and the wind pretty strong at north; and I believe this the coldest evening we have had this fall, though this week we had ice as thick as a dollar.

Extract of the Answer to the above Letter.

London, 11 January, 1758.

I THANK you for your account of the Aurora. A very considerable one appeared here the same evening, being Saturday, November 12th. I did not see it, but have heard of it from several. If it was the same, that you saw, it must have been very high, or very extensive, as the two places are one thousand leagues asunder.

TO JOHN PRINGLE.

On the different Strata of the Earth.

Craven Street, 6 January, 1758.

SIR,

I return you Mr. Mitchell's paper on the strata of the earth* with thanks. The reading of it, and perusal of the draft that accompanies it, have reconciled me to those convulsions which all naturalists agree this globe has suffered. Had the different strata of clay, gravel, marble, coals, limestone, sand, minerals, &c., continued to lie level, one under the other, as they may be supposed to have done before those convulsions, we should have had the use only of a few of the uppermost of the strata, the others lying too deep and too difficult to be come at; but, the shell of the earth being broke, and the fragments thrown into this oblique position, the disjointed ends of a great number of strata of different kinds are brought up to day, and a great variety of useful materials put into our power, which would otherwise have remained eternally concealed from us. So that what has been usually looked upon as a *ruin* suffered by this part of the universe, was, in reality, only a preparation, or means of rendering the earth more fit for use, more capable of being to mankind a convenient and comfortable habitation.

I am, Sir, with great esteem, yours, &c.

B. FRANKLIN.

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* See this paper, afterwards printed in the *Philosophical Transactions*.

TO JOHN LINING, AT CHARLESTON.

Experiment with Ether for producing Cold by Evaporation.—Water kept cool in hot Countries.—The human Body cooled by Evaporation.—The Surface of the Earth and Vegetation cooled in the same Manner.

London, 17 June, 1758.

DEAR SIR,

In a former letter I mentioned the experiment for cooling bodies by evaporation, and that I had, by repeatedly wetting the thermometer with common spirits, brought the mercury down five or six degrees. Being lately at Cambridge, and mentioning this in conversation with Dr. Hadley, professor of chemistry there, he proposed repeating the experiments with ether, instead of common spirits, as the ether is much quicker in evaporation. We accordingly went to his chamber, where he had both ether and a thermometer. By dipping first the ball of the thermometer into the ether, it appeared that the ether was precisely of the same temperament with the thermometer, which stood then at 65; for it made no alteration in the height of the little column of mercury. But, when the thermometer was taken out of the ether, and the ether, with which the ball was wet, began to evaporate, the mercury sunk several degrees. The wetting was then repeated by a feather that had been dipped into the ether, when the mercury sunk still lower.

We continued this operation, one of us wetting the ball, and another of the company blowing on it with the bellows, to quicken the evaporation, the mercury sinking all the time, till it came down to 7, which is 25 degrees below the freezing point, when we left off.

Soon after it passed the freezing point, a thin coat of ice began to cover the ball. Whether this was water collected and condensed by the coldness of the ball, from the moisture in the air, or from our breath; or whether the feather, when dipped into the ether, might not sometimes go through it, and bring up some of the water that was under it, I am not certain; perhaps all might contribute. The ice continued increasing till we ended the experiment, when it appeared near a quarter of an inch thick all over the ball, with a number of small *spicula*, pointing outwards. From this experiment one may see the possibility of freezing a man to death on a warm summer's day, if he were to stand in a passage through which the wind blew briskly, and to be wet frequently with ether, a spirit that is more inflammable than brandy or common spirits of wine.

It is but within these few years, that the European philosophers seem to have known this power in nature, of cooling bodies by evaporation. But in the east they have long been acquainted with it. A friend tells me, there is a passage in Bernier's Travels through Indostan, written near one hundred years ago, that mentions it as a practice (in travelling over dry deserts in that hot climate) to carry water in flasks wrapped in wet woollen cloths, and hung on the shady side of the camel, or carriage, but in the free air; whereby, as the cloths gradually grow drier, the water contained in the flasks is made cool. They have likewise a kind of earthen pots, unglazed, which let the water gradually and slowly ooze through their pores, so as to keep the outside a little wet, notwithstanding the continual evaporation, which gives great coldness to the vessel, and the water contained in it. Even our common sailors seem to have had some notion of this property; for I remember, that, being at sea when I was a youth, I observed

one of the sailors, during a calm in the night, often wetting his finger in his mouth, and then holding it up in the air, to discover, as he said, if the air had any motion, and from which side it came; and this he expected to do, by finding one side of his finger grow suddenly cold, and from that side he should look for the next wind; which I then laughed at as a fancy.

May not several phenomena, hitherto unconsidered, or unaccounted for, be explained by this property? During the hot Sunday at Philadelphia, in June, 1750, when the thermometer was up at 100 in the shade, I sat in my chamber without exercise, only reading or writing, with no other clothes on than a shirt, and a pair of long linen drawers, the windows all open, and a brisk wind blowing through the house; the sweat ran off the backs of my hands, and my shirt was often so wet, as to induce me to call for dry ones to put on. In this situation, one might have expected, that the natural heat of the body 96, added to the heat of the air 100, should jointly have created or produced a much greater degree of heat in the body; but the fact was, that my body never grew so hot as the air that surrounded it, or the inanimate bodies immersed in the same air. For I remember well, that the desk, when I laid my arm upon it; a chair, when I sat down in it; and a dry shirt out of the drawer, when I put it on, all felt exceeding warm to me, as if they had been warmed before a fire. And I suppose a dead body would have acquired the temperature of the air, though a living one, by continual sweating, and by the evaporation of that sweat, was kept cold.

May not this be a reason why our reapers in Pennsylvania, working in the open field in the clear hot sunshine common in our harvest-time,* find them-

* Pennsylvania is in about lat. 40, and the sun, of course, about 12

selves well able to go through that labor, without being much incommoded by the heat, while they continue to sweat, and while they supply matter for keeping up that sweat, by drinking frequently of a thin evaporable liquor, water mixed with rum; but, if the sweat stops, they drop, and sometimes die suddenly, if a sweating is not again brought on by drinking that liquor, or, as some rather choose in that case, a kind of hot punch, made with water, mixed with honey, and a considerable proportion of vinegar? May there not be in negroes a quicker evaporation of the perspirable matter from their skins and lungs, which, by cooling them more, enables them to bear the sun's heat better than whites do? (if that is a fact, as it is said to be; for the* alleged necessity of having negroes rather than whites, to work in the West India fields, is founded upon it,) though the color of their skins would otherwise make them more sensible of the sun's heat, since black cloth heats much sooner and more, in the sun, than white cloth. I am persuaded, from several instances happening within my knowledge, that they do not bear cold weather so well as the whites; they will perish when exposed to a less degree of it, and are more apt to have their limbs frost-bitten; and may not this be from the same cause?

Would not the earth grow much hotter under the summer sun, if a constant evaporation from its surface, greater as the sun shines stronger, did not, by tending to cool it, balance, in some degree, the warmer effects of the sun's rays? Is it not owing to the constant evaporation from the surface of every leaf, that trees, though shone on by the sun, are always, even the

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degrees higher, and therefore much hotter, than in England. Their harvest is about the end of June, or beginning of July, when the sun is nearly at the highest.

leaves themselves, cool to our sense? at least, much cooler than they would otherwise be? May it not be owing to this, that fanning ourselves when warm, does really cool us, though the air is itself warm that we drive with the fan upon our faces? For the atmosphere, round and next to our bodies, having imbibed as much of the perspired vapor as it can well contain, receives no more, and the evaporation is therefore checked and retarded, till we drive away that atmosphere, and bring drier air in its place, that will receive the vapor, and thereby facilitate and increase the evaporation? Certain it is, that mere blowing of air on a dry body does not cool it, as any one may satisfy himself, by blowing with a bellows on the dry ball of a thermometer; the mercury will not fall; if it moves at all, it rather rises, as being warmed by the friction of the air on its surface.

To these queries of imagination, I will only add one practical observation; that, wherever it is thought proper to give ease, in cases of painful inflammation in the flesh (as from burnings or the like), by cooling the part, linen cloths wet with spirit, and applied to the part inflamed, will produce the coolness required, better than if wet with water, and will continue it longer. For water, though cold when first applied, will soon acquire warmth from the flesh, as it does not evaporate fast enough; but the cloths wet with spirit, will continue cold as long as any spirit is left to keep up the evaporation, the parts warmed escaping as soon as they are warmed, and carrying off the heat with them.

I am, Sir, &c.

B. FRANKLIN.

TO PETER FRANKLIN,* AT NEWPORT.

On the Saltness of Sea-Water.

London, 7 May, 1760.

—— It has, indeed, as you observe, been the opinion of some very great naturalists, that the sea is salt only from the dissolution of mineral or rock salt, which its waters happened to meet with. But this opinion takes it for granted, that all water was originally fresh, of which we can have no proof. I own I am inclined to a different opinion, and rather think all the water on this globe was originally salt, and that the fresh water we find in springs and rivers, is the produce of distillation. The sun raises the vapors from the sea, which form clouds, and fall in rain upon the land, and springs and rivers are formed of that rain. As to the rock salt found in mines, I conceive, that, instead of communicating its saltness to the sea, it is itself drawn from the sea, and that of course the sea is now fresher than it was originally. This is only another effect of nature's distillery, and might be performed various ways.

It is evident from the quantities of sea-shells, and the bones and teeth of fishes found in high lands, that the sea has formerly covered them. Then, either the sea has been higher than it now is, and has fallen away from those high lands, or they have been lower than they are, and were lifted up out of the water to their present height, by some internal mighty force, such as we still feel some remains of, when whole continents are moved by earthquakes. In either case it may be supposed, that large hollows, or valleys among hills,

* An elder brother of the author, who resided many years at Newport, in Rhode Island. — Editor.

might be left filled with sea-water, which evaporating, and the fluid part drying away in a course of years, would leave the salt covering the bottom; and that salt, coming afterwards to be covered with earth from the neighbouring hills, could only be found by digging through that earth. Or, as we know from their effects, that there are deep fiery caverns under the earth, and even under the sea, if at any time the sea leaks into any of them, the fluid parts of the water must evaporate from that heat, and pass off through some volcano, while the salt remains, and by degrees, and continual accretion, becomes a great mass. Thus the cavern may at length be filled, and the volcano connected with it cease burning, as many it is said have done; and future miners, penetrating such cavern, find what we call a salt-mine. This is a fancy I had on visiting the salt-mines at Northwich, with my son. I send you a piece of the rock salt which he brought up with him out of the mine. I am, &c.

B. FRANKLIN.

TO ALEXANDER SMALL, LONDON.

On the Northeast Storms in North America.

12 May, 1760.

DEAR SIR,

Agreeably to your request, I send you my reasons for thinking that our northeast storms in North America begin first, in point of time, in the southwest parts; that is to say, the air in Georgia, the farthest of our colonies to the southwest, begins to move southwesterly before the air of Carolina, which is the next colony northeastward; the air of Carolina has the same motion

before the air of Virginia, which lies still more northeastward; and so on northeasterly through Pennsylvania, New York, New England, &c., quite to Newfoundland.

These northeast storms are generally very violent, continue sometimes two or three days, and often do considerable damage in the harbours along the coast. They are attended with thick clouds and rain.

What first gave me this idea, was the following circumstance. About twenty years ago, a few more or less, I cannot from my memory be certain, we were to have an eclipse of the moon at Philadelphia, on a Friday evening, about nine o'clock. I intended to observe it, but was prevented by a northeast storm, which came on about seven, with thick clouds as usual, that quite obscured the whole hemisphere. Yet when the post brought us the Boston newspaper, giving an account of the effects of the same storm in those parts, I found the beginning of the eclipse had been well observed there, though Boston lies northeast of Philadelphia about four hundred miles. This puzzled me, because the storm began with us so soon as to prevent any observation, and, being a northeast storm, I imagined it must have begun rather sooner in places farther to the northeastward than it did at Philadelphia. I therefore mentioned it in a letter to my brother, who lived at Boston; and he informed me the storm did not begin with them till near eleven o'clock, so that they had a good observation of the eclipse; and upon comparing all the other accounts I received from the several colonies, of the time of beginning of the same storm, and, since that, of other storms of the same kind, I found the beginning to be always later the farther northeastward. I have not my notes with me here in England, and cannot, from memory, say the proportion of time

to distance, but I think it is about an hour to every hundred miles.*

From thence I formed an idea of the cause of these storms, which I would explain by a familiar instance or two. Suppose a long canal of water stopped at the end by a gate. The water is quite at rest till the gate is open, then it begins to move out through the gate; the water next the gate is first in motion, and moves towards the gate; the water next to that first water moves next, and so on successively, till the water at the head of the canal is in motion, which is last of all. In this case, all the water moves indeed towards the gate, but the successive times of beginning motion are the contrary way, viz. from the gate backwards to the head of the canal. Again, suppose the air in a chamber at rest, no current through the room till you make a fire in the chimney. Immediately the air in the chimney, being rarefied by the fire, rises; the air next the chimney flows in to supply its place, moving towards the chimney; and, in consequence, the rest of the air successively, quite back to the door. Thus to produce our northeast storms, I suppose some great heat and rarefaction of the air in or about the Gulf of Mexico; the air thence rising has its place supplied by the next more northern, cooler, and therefore denser and heavier, air; that, being in motion, is followed by the next more northern air, &c. &c., in a successive current, to which current our coast and inland ridge of mountains give the direction of northeast, as they lie northeast and southwest.

This I offer only as an hypothesis to account for this particular fact; and perhaps, on farther examination, a better and truer may be found. I do not suppose all

* See a more full account of this subject above, (p. 105,) in a letter to the Reverend Jared Eliot. — Editor.

storms generated in the same manner. Our northwest thunder-gusts in America, I know are not; but of them I have written my opinion fully in a paper which you have seen.

I am, &c.

B. FRANKLIN.

TO MISS MARY STEVENSON.

On the Effect of Air on the Barometer, and the Benefits derived from the Study of Insects.

Craven Street, 11 June, 1760.

IT is a very sensible question you ask, how the air can affect the barometer, when its opening appears covered with wood? If indeed it was so closely covered as to admit of no communication of the outward air to the surface of the mercury, the change of weight in the air could not possibly affect it. But the least crevice is sufficient for the purpose; a pinhole will do the business. And if you could look behind the frame to which your barometer is fixed, you would certainly find some small opening.

There are indeed some barometers in which the body of mercury at the lower end is contained in a close leather bag, and so the air cannot come into immediate contact with the mercury; yet the same effect is produced. For, the leather being flexible, when the bag is pressed by any additional weight of air, it contracts, and the mercury is forced up into the tube; when the air becomes lighter, and its pressure less, the weight of the mercury prevails, and it descends again into the bag.

Your observation on what you have lately read concerning insects is very just and solid. Superficial minds are apt to despise those who make that part of the creation their study, as mere triflers; but certainly the world has been much obliged to them. Under the care and management of man, the labors of the little silkworm afford employment and subsistence to thousands of families, and become an immense article of commerce. The bee, too, yields us its delicious honey, and its wax useful to a multitude of purposes. Another insect, it is said, produces the cochineal, from whence we have our rich scarlet dye. The usefulness of the cantharides, or Spanish flies, in medicine, is known to all, and thousands owe their lives to that knowledge. By human industry and observation, other properties of other insects may possibly be hereafter discovered, and of equal utility. A thorough acquaintance with the nature of these little creatures may also enable mankind to prevent the increase of such as are noxious, or secure us against the mischiefs they occasion. These things doubtless your books make mention of; I can only add a particular late instance which I had from a Swedish gentleman of good credit. In the green timber, intended for ship-building at the King's yards in that country, a kind of worms were found, which every year became more numerous and more pernicious, so that the ships were greatly damaged before they came into use. The King sent Linnæus, the great naturalist, from Stockholm, to inquire into the affair, and see if the mischief was capable of any remedy. He found, on examination, that the worm was produced from a small egg, deposited in the little roughnesses on the surface of the wood, by a particular kind of fly or beetle; from whence the worm, as soon as it was hatched, began to eat into the substance of

the wood, and after some time came out again a fly of the parent kind, and so the species increased. The season in which the fly laid its eggs, Linnæus knew to be about a fortnight (I think) in the month of May, and at no other time in the year. He therefore advised, that, some days before that season, all the green timber should be thrown into the water, and kept under water till the season was over. Which being done by the King's order, the flies missing their usual nests, could not increase; and the species was either destroyed or went elsewhere; and the wood was effectually preserved; for, after the first year, it became too dry and hard for their purpose.

There is, however, a prudent moderation to be used in studies of this kind. The knowledge of nature may be ornamental, and it may be useful; but if, to attain an eminence in that, we neglect the knowledge and practice of essential duties, we deserve reprehension. For there is no rank in natural knowledge of equal dignity and importance with that of being a good parent, a good child, a good husband or wife, a good neighbour or friend, a good subject or citizen, that is, in short, a good Christian. Nicholas Gimcrack, therefore, who neglected the care of his family, to pursue butterflies, was a just object of ridicule, and we must give him up as fair game to the satirist.

Adieu, my dear friend, and believe me ever

Yours affectionately,

B. FRANKLIN.

TO MISS MARY STEVENSON.

On the Bristol Waters, and the Tide in Rivers.

London, 13 September, 1760.

MY DEAR FRIEND,

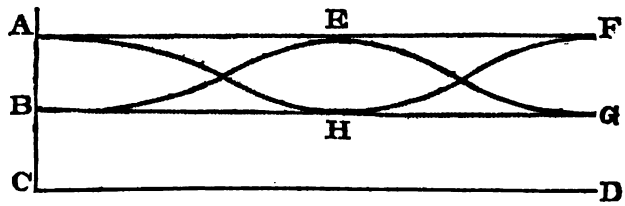
I have your agreeable letter from Bristol, which I take this first leisure hour to answer, having for some time been much engaged in business.

Your first question, *What is the reason the water at this place, though cold at the spring, becomes warm by pumping?* it will be most prudent in me to forbear attempting to answer, till, by a more circumstantial account, you assure me of the fact. I own I should expect that operation to warm, not so much the water pumped, as the person pumping. The rubbing of dry solids together has been long observed to produce heat; but the like effect has never yet, that I have heard, been produced by the mere agitation of fluids, or friction of fluids with solids. Water in a bottle, shook for hours by a mill-hopper, it is said, discovered no sensible addition of heat. The production of animal heat by exercise is therefore to be accounted for in another manner, which I may hereafter endeavour to make you acquainted with.

This prudence of not attempting to give reasons before one is sure of facts, I learned from one of your sex, who, as Selden tells us, being in company with some gentlemen that were viewing and considering something which they called a Chinese shoe, and disputing earnestly about the manner of wearing it, and how it could possibly be put on; put in her word, and said modestly, *Gentlemen, are you sure it is a shoe? Should not that be settled first?*

But I shall now endeavour to explain what I said to

you about the tide in rivers, and to that end shall make a figure, which, though not very like a river, may serve to convey my meaning. Suppose a canal one hundred and forty miles long, communicating at one end with the sea, and filled therefore with sea water. I choose a canal at first, rather than a river, to throw out of consideration the effects produced by the streams of fresh water from the land, the inequality in breadth, and the crookedness of courses.



Let A, C, be the head of the canal; C, D, the bottom of it; D, F, the open mouth of it, next the sea. Let the straight pricked line, B, G, represent low-water mark, the whole length of the canal, A, F, high-water mark. Now if a person, standing at E, and observing, at the time of high water there, that the canal is quite full at that place up to the line E, should conclude that the canal is equally full to the same height from end to end, and therefore there was as much more water come into the canal since it was down at low-water mark, as would be included in the oblong space A, B, G, F, he would be greatly mistaken. For the tide is *a wave*, and the top of the wave, which makes high water, as well as every other lower part, is progressive; and it is high water successively, but not at the same time, in all the several points between G, F, and A, B. And in such a length as I have mentioned, it is low water at F, G, and also at A, B, at or near the same time with its being high water at E; so that the surface of the water in the canal, during that situation, is prop-

erly represented by the curve pricked line, B, E, G. And on the other hand, when it is low water at E, H, it is high water both at F, G, and at A, B, at or near the same time; and the surface would then be described by the inverted curve line, A, H, F.

In this view of the case, you will easily see, that there must be very little more water in the canal at what we call high water, than there is at low water, those terms not relating to the whole canal at the same time, but successively to its parts. And, if you suppose the canal six times as long, the case would not vary as to the quantity of water at different times of the tide; there would only be six waves in the canal at the same time, instead of one, and the hollows in the water would be equal to the hills.

That this is not mere theory, but conformable to fact, we know by our long rivers in America. The Delaware, on which Philadelphia stands, is in this particular similar to the canal I have supposed of one wave; for, when it is high water at the Capes or mouth of the river, it is also high water at Philadelphia, which stands about one hundred and forty miles from the sea; and there is at the same time a low water in the middle between the two high waters; where, when it comes to be high water, it is at the same time low water at the Capes and at Philadelphia. And the longer rivers have some a wave and half, some two, three, or four waves, according to their length. In the shorter rivers of this island, one may see the same thing in part; for instance, it is high water at Gravesend an hour before it is high water at London Bridge; and twenty miles below Gravesend an hour before it is high water at Gravesend. Therefore at the time of high water at Gravesend the top of the wave is there, and the water is then not so high by some feet where

the top of the wave was an hour before, or where it will be an hour after, as it is just then at Gravesend.

Now we are not to suppose, because the swell or top of the wave runs at the rate of twenty miles an hour, that therefore the current, or water itself of which the wave is composed, runs at that rate. Far from it. To conceive this motion of a wave, make a small experiment or two. Fasten one end of a cord in a window near the top of a house, and let the other end come down to the ground; take this end in your hand, and you may, by a sudden motion, occasion a wave in the cord that will run quite up to the window; but though the wave is progressive from your hand to the window, the parts of the rope do not proceed with the wave, but remain where they were, except only that kind of motion that produces the wave. So if you throw a stone into a pond of water when the surface is still and smooth, you will see a circular wave proceed from the stone as its centre, quite to the sides of the pond; but the water does not proceed with the wave, it only rises and falls to form it in the different parts of its course; and the waves that follow the first, all make use of the same water with their predecessors.

But a wave in water is not indeed in all circumstances exactly like that in a cord; for, water being a fluid, and gravitating to the earth, it naturally runs from a higher place to a lower; therefore the parts of the wave in water do actually run a little both ways from its top towards its lower sides, which the parts of the wave in the cord cannot do. Thus, when it is high and standing water at Gravesend, the water twenty miles below has been running ebb, or towards the sea for an hour, or ever since it was high water there; but the water at London Bridge will run flood, or from the sea yet another hour, till it is high water,

or the top of the wave arrives at that bridge, and then it will have run ebb an hour at Gravesend, &c. &c. Now this motion of the water, occasioned only by its gravity, or tendency to run from a higher place to a lower, is by no means so swift as the motion of the wave. It scarce exceeds perhaps two miles in an hour.

If it went, as the wave does, twenty miles an hour, no ships could ride at anchor in such a stream, nor boats row against it.

In common speech, indeed, this current of the water both ways from the top of the wave is called *the tide;* thus we say, *the tide runs strong, the tide runs at the rate of one, two, or three miles an hour, &c.*; and, when we are at a part of the river behind the top of the wave, and find the water lower than high-water mark, and running towards the sea, we say, *the tide runs ebb;* and, when we are before the top of the wave, and find the water higher than low-water mark, and running from the sea, we say, *the tide runs flood;* but these expressions are only locally proper; for a tide, strictly speaking, is *one whole wave*, including all its parts higher and lower, and these waves succeed one another about twice in twenty-four hours.

This motion of the water, occasioned by its gravity, will explain to you why the water near the mouths of rivers may be salter at high water than at low. Some of the salt water, as the tide wave enters the river, runs from its top and fore side, and mixes with the fresh, and also pushes it back up the river.

Supposing that the water commonly runs during the flood at the rate of two miles in an hour, and that the flood runs five hours, you see that it can bring at most into our canal only a quantity of water equal to the space included in the breadth of the canal, ten miles of its length, and the depth between low and high-

water mark; which is but a fourteenth part of what would be necessary to fill all the space between low and high-water mark for one hundred and forty miles, the whole length of the canal.

And indeed such a quantity of water as would fill that whole space, to run in and out every tide, must create so outrageous a current, as would do infinite damage to the shores, shipping, &c., and make the navigation of a river almost impracticable.

I have made this letter longer than I intended, and therefore reserve for another what I have further to say on the subject of tides and rivers. I shall now only add, that I have not been exact in the numbers, because I would avoid perplexing you with minute calculations, my design at present being chiefly to give you distinct and clear ideas of the first principles.

After writing six folio pages of philosophy to a young girl, is it necessary to finish such a letter with a compliment? Is not such a letter of itself a compliment? Does it not say, she has a mind thirsty after knowledge, and capable of receiving it; and that the most agreeable things one can write to her are those that tend to the improvement of her understanding? It does indeed say all this, but then it is still no compliment; it is no more than plain honest truth, which is not the character of a compliment. So if I would finish my letter in the *mode*, I should yet add something that means nothing, and is *merely* civil and polite. But, being naturally awkward at every circumstance of ceremony, I shall not attempt it. I had rather conclude abruptly with what pleases me more than any compliment can please you, that I am allowed to subscribe myself

Your affectionate friend,

B. FRANKLIN.

TO MISS MARY STEVENSON.

Tides in Rivers.

Craven Street, Monday, 30 March, 1761.

My dear Friend,

Supposing the fact, that the water of the well at Bristol is warmer after some time pumping, I think your manner of accounting for that increased warmth very ingenious and probable. It did not occur to me, and therefore I doubted of the fact.

You are, I think, quite right in your opinion, that the rising of the tides in rivers is not owing to the immediate influence of the moon on the rivers. It in rather a subsequent effect of the influence of the moon on the sea, and does not make its appearance in some rivers till the moon has long passed by. I have not expressed myself clearly, if you have understood me to mean otherwise. You know I have mentioned it as a fact, that there are in some rivers several tides all existing at the same time; that is, two, three, or more high-waters, and as many low-waters, in different parts of the same river, which cannot possibly be all effects of the moon's immediate action on that river; but they may be subsequent effects of her action on the sea.

In the enclosed paper you will find my sentiments on several points relating to the air, and the evaporation of water. It is Mr. Collinson's copy, who took it from one I sent through his hands to a correspondent in France some years since; I have, as he desired me, corrected the mistakes he made in transcribing, and must return it to him; but if you think it worth while, you may take a copy of it. I would have saved you any trouble of that kind, but had not time.

Some day in the next or the following week, I purpose to have the pleasure of seeing you at Wanstead. I shall accompany your good mamma thither, and stay till the next morning, if it may be done without incommoding your family too much. We may then discourse any points in that paper that do not seem clear to you; and, taking a walk to Lord Tilney's ponds, make a few experiments there to explain the nature of the tides more fully. In the mean time, believe me to be, with the highest esteem and regard,

Your sincerely affectionate friend,

B. FRANKLIN.

TO MISS MARY STEVENSON.

Salt Water rendered fresh by Distillation. — Method of relieving Thirst by Sea Water.

Craven Street, 10 August, 1761.

WE are to set out this week for Holland, where we may possibly spend a month, but purpose to be at home again before the coronation. I could not go without taking leave of you by a line at least, when I am so many letters in your debt.

In yours of May 19th, which I have before me, you speak of the ease with which salt water may be made fresh by distillation, supposing it to be, as I had said, that in evaporation the air would take up water, but not the salt that was mixed with it. It is true, that distilled sea water will not be salt, but there are other disagreeable qualities that rise with the water in distillation; which indeed several besides Dr. Hales have endeavoured by some means to prevent; but as yet their methods have not been brought much into use.

I have a singular opinion on this subject, which I will venture to communicate to you, though I doubt you will rank it among my whims. It is certain that the skin has *imbibing* as well as *discharging* pores; witness the effects of a blistering plaster, &c. I have read, that a man, hired by a physician to stand by way of experiment in the open air naked during a moist night, weighed near three pounds heavier in the morning. I have often observed myself, that, however thirsty I may have been before going into the water to swim, I am never long so in the water. These imbibing pores, however, are very fine, perhaps fine enough in filtering to separate salt from water; for, though I have soaked (by swimming, when a boy,) several hours in the day for several days successively in salt water, I never found my blood and juices salted by that means, so as to make me thirsty or feel a salt taste in my mouth; and it is remarkable, that the flesh of sea fish, though bred in salt water, is not salt.

Hence I imagine, that, if people at sea, distressed by thirst when their fresh water is unfortunately spent, would make bathing-tubs of their empty water-casks, and, filling them with sea water, sit in them an hour or two each day, they might be greatly relieved. Perhaps keeping their clothes constantly wet might have an almost equal effect; and this without danger of catching cold. Men do not catch cold by wet clothes at sea. Damp, but not wet linen may possibly give colds; but no one catches cold by bathing, and no clothes can be wetter than water itself. Why damp clothes should then occasion colds, is a curious question, the discussion of which I reserve for a future letter, or some future conversation.

Adieu, my little philosopher. Present my respectful

compliments to the good ladies, your aunts, and to Miss Pitt; and believe me ever

Your affectionate friend,
And humble Servant,
B. FRANKLIN.

TO MISS MARY STEVENSON.

Tendency of Rivers to the Sea. — Effect of the Sun's Rays on Cloths of different Colors.

20 September, 1761.

MY DEAR FRIEND,

It is, as you observed in our late conversation, a very general opinion, that *all rivers run into the sea*, or deposite their waters there. It is a kind of audacity to call such general opinions in question, and may subject one to censure. But we must hazard something in what we think the cause of truth; and if we propose our objections modestly, we shall, though mistaken, deserve a censure less severe, than when we are both mistaken and insolent.

That some rivers run into the sea is beyond a doubt; such, for instance, are the Amazons and, I think, the Oronoko and the Mississippi. The proof is, that their waters are fresh quite to the sea, and out to some distance from the land. Our question is, whether the fresh waters of those rivers whose beds are filled with salt water to a considerable distance up from the sea (as the Thames, the Delaware, and the rivers that communicate with Chesapeake Bay in Virginia) do ever arrive at the sea. And as I suspect they do not, I am now to acquaint you with my reasons; or, if they are not allowed to be reasons, my conceptions, at least, of this matter.

The common supply of rivers is from springs, which draw their origin from rain that has soaked into the earth. The union of a number of springs forms a river. The waters, as they run, exposed to the sun, air, and wind, are continually evaporating. Hence in travelling one may often see where a river runs, by a long blueish mist over it, though we are at such a distance as not to see the river itself. The quantity of this evaporation is greater or less, in proportion to the surface exposed by the same quantity of water to those causes of evaporation. While the river runs in a narrow confined channel in the upper hilly country, only a small surface is exposed; a greater, as the river widens. Now, if a river ends in a lake, as some do, whereby its waters are spread so wide as that the evaporation is equal to the sum of all its springs, that lake will never overflow; And if, instead of ending in a lake, it was drawn into greater length as a river, so as to expose a surface equal in the whole to that lake, the evaporation would be equal, and such river would end as a canal; when the ignorant might suppose, as they actually do in such cases, that the river loses itself by running under ground, whereas in truth it has run up into the air.

Now, many rivers that are open to the sea widen much before they arrive at it, not merely by the additional waters they receive, but by having their course stopped by the opposing flood-tide; by being turned back twice in twenty-four hours, and by finding broader beds in the low flat countries to dilate themselves in. Hence the evaporation of the fresh water is proportionably increased; so that in some rivers it may equal the springs of supply. In such cases, the salt water comes up the river, and meets the fresh in that part where, if there were a wall or bank of earth across from side to side, the river would form a lake, fuller indeed at

some times than at others, according to the seasons, but whose evaporation would, one time with another, be equal to its supply.

When the communication between the two kinds of water is open, this supposed wall of separation may be conceived as a movable one, which is not only pushed some miles higher up the river by every flood tide from the sea, and carried down again as far by every tide of ebb, but which has even this space of vibration removed nearer to the sea in wet seasons, when the springs and brooks in the upper country are augmented by the falling rains, so as to swell the river, and farther from the sea in dry seasons.

Within a few miles above and below this movable line of separation, the different waters mix a little, partly by their motion to and fro, and partly from the greater specific gravity of the salt water, which inclines it to run under the fresh, while the fresh water, being lighter, runs over the salt.

Cast your eye on the map of North America, and observe the Bay of Chesapeake, in Virginia, mentioned above; you will see, communicating with it by their mouths, the great rivers Susquehanna, Potomac, Rappahanoc, York, and James, besides a number of smaller streams, each as big as the Thames. It has been proposed by philosophical writers, that to compute how much water any river discharges into the sea in a given time, we should measure its depth and swiftness at any part above the tide; as, for the Thames, at Kingston or Windsor. But can one imagine, that if all the water of those vast rivers went to the sea, it would not first have pushed the salt water out of that narrow-mouthed bay, and filled it with fresh? The Susquehanna alone would seem to be sufficient for this, if it were not for the loss by evap-

oration. And yet that bay is salt quite up to Annapolis.

As to our other subject, the different degrees of heat imbibed from the sun's rays by cloths of different colors, since I cannot find the notes of my experiment to send you, I must give it as well as I can from memory.

But, first, let me mention an experiment you may easily make yourself. Walk but a quarter of an hour in your garden when the sun shines, with a part of your dress white, and a part black; then apply your hand to them alternately, and you will find a very great difference in their warmth. The black will be quite hot to the touch, the white still cool.

Another. Try to fire the paper with a burning glass. If it is white, you will not easily burn it; but if you bring the focus to a black spot, or upon letters, written or printed, the paper will immediately be on fire under the letters.

Thus fullers and dyers find black cloths, of equal thickness with white ones, and hung out equally wet, dry in the sun much sooner than the white, being more readily heated by the sun's rays. It is the same before a fire; the heat of which sooner penetrates black stockings than white ones, and so is apt sooner to burn a man's shins. Also beer much sooner warms in a black mug set before the fire, than in a white one, or in a bright silver tankard.

My experiment was this. I took a number of little square pieces of broad cloth from a tailor's pattern-card, of various colors. There were black, deep blue, lighter blue, green, purple, red, yellow, white, and other colors, or shades of colors. I laid them all out upon the snow in a bright sunshiny morning. In a few hours (I cannot now be exact as to the time), the black,

being warmed most by the sun, was sunk so low as to be below the stroke of the sun's rays; the dark blue almost as low, the lighter blue not quite so much as the dark, the other colors less as they were lighter; and the quite white remained on the surface of the snow, not having entered it at all.

What signifies philosophy that does not apply to some use? May we not learn from hence, that black clothes are not so fit to wear in a hot, sunny climate or season, as white ones; because in such clothes the body is more heated by the sun when we walk abroad, and are at the same time heated by the exercise, which double heat is apt to bring on putrid, dangerous fevers? That soldiers and seamen, who must march and labor in the sun, should in the East or West Indies have an uniform of white? That summer hats, for men or women, should be white, as repelling that heat which gives headaches to many, and to some the fatal stroke that the French call the *coup de soleil?* That the ladies' summer hats, however, should be lined with black, as not reverberating on their faces those rays which are reflected upwards from the earth or water? That the putting a white cap of paper or linen *within* the crown of a black hat, as some do, will not keep out the heat, though it would if placed *without?* That fruit-walls being blacked may receive so much heat from the sun in the daytime, as to continue warm in some degree through the night, and thereby preserve the fruit from frosts, or forward its growth? — with sundry other particulars of less or greater importance, that will occur from to time to attentive minds? I am

Yours affectionately,

B. FRANKLIN.

On Fire.

Craven Street, 1762.

Did you ever see people at work with spades and pickaxes, digging a cellar? When they have loosened the earth perhaps a foot deep, that loose earth must be carried off, or they can go no deeper; it is in their way, and hinders the operation of the instruments.

When the first foot of earth is removed, they can dig and loosen the earth a foot deeper. But if those who remove the earth should with it take away the spades and pickaxes, the work will be equally obstructed as if they had left the loose earth unremoved.

I imagine the operation of fire upon fuel with the assistance of air may be in some degree similar to this. Fire penetrates bodies, and separates their parts; the air receives and carries off the parts separated, which, if not carried off, would impede the action of the fire. With this assistance therefore of a moderate current of air, the separation increases, but too violent a blast carries off the fire itself; and thus any fire may be blown out, as a candle by the breath, if the blast be proportionable.

But, if air contributed inflammatory matter, as some have thought, then it should seem, that, the more air, the more the flame would be augmented, which beyond certain bounds does not agree with the fact.

Some substances take fire, that is, are kindled by the application of fire, much sooner than others. This is in proportion as they are good or bad conductors of fire, and as their parts cohere with less or more strength. A bad conductor of fire not easily permitting it to penetrate and be absorbed, and its force divided among the whole substance, its operation is so much the

stronger on the surface to which it is applied, and is in a small depth of surface strong enough to produce the separation of parts which we call *burning*. All oils and fats, wax, sulphur, and most vegetable substances, are bad conductors of fire. The oil of a lamp, burning at the top, may be scarce warm at the bottom; a candle or a stick of wood, inflamed at one end, is cool at the other. Metals, which are better conductors, are not so easily kindled, though, when sufficient fire is applied to them to separate their parts, they will all burn. But the fire applied to their surfaces enters more easily, is absorbed and divided; and not enough left on the surface to overcome the cohesion of their parts. A close contact with metals will for the same reason prevent the burning of more inflammable substances. A flaxen thread, bound close round an iron poker, will not burn in the flame of a candle; for it must imbibe a certain quantity of fire before it can burn, that is, before its parts can separate; but the poker, as fast as the fire arrives, takes it from the thread, conducts it away, and divides it in its own substance.

Common fire I conceive to be collected by friction from the common mass of that fluid, in the same manner as the electrical fluid is collected by friction, which I have endeavoured to explain in some of my electrical papers, and, to avoid length in this letter, refer you to them. In wheels, the particles of grease and oil acting as so many little rollers, and preventing friction between the wood and wood, do thereby prevent the collection of fire.

TO DAVID HUME.*

On the Method of securing Houses from the Effects of Lightning.

London, 24 January, 1762.

DEAR SIR,

In compliance with my Lord Marischal's request, communicated to me by you, when I last had the pleasure of seeing you, I now send you what at present appears to me to be the shortest and simplest method of securing buildings, &c., from the mischiefs of lightning. Prepare a steel rod five or six feet long, half an inch thick at its biggest end, and tapering to a sharp point; which point should be gilt to prevent its rusting. Let the big end of the rod have a strong eye or ring of half an inch diameter: Fix this rod upright to the chimney or highest part of the house, by means of staples, so as it may be kept steady. Let the pointed end be upwards, and rise three or four feet above the chimney or building that the rod is fixed to. Drive into the ground an iron rod of about an inch diameter, and ten or twelve feet long, that has also an eye or ring in its upper end. It is best that the rod should be at some distance from the foundation of the building, not nearer than ten feet, if your ground will allow so much. Then take as much length of iron rod of about half an inch diameter, as will reach from the eye in the rod above, to that in the rod below; and fasten it securely to those rods, by passing its ends through the rings, and bending those ends till they likewise form rings.

* First printed in the third volume of a work entitled, "*Essays and Observations, Physical and Literary; read before the Philosophical Society in Edinburgh, and published by them.*" Mr. Hume was a member of that Society. The volume containing the above letter was published in the year 1771. — EDITOR.

This length of rod may either be in one or several pieces. If in several, let the ends of the pieces be also well hooked to each other. Then close and cover every joint with lead, which is easily done, by making a small bag of strong paper round the joint, tying it close below, and then pouring in the melted lead; it being of use in these junctures, that there should be a considerable quantity of metalline contact between piece and piece. For, if they were only hooked together, and so touched each other but in points, the lightning, in passing through them, might melt and break them where they join. The lead will also prevent the weakening of the joints by rust. To prevent the shaking of this rod by the wind, you may secure it by a few staples to the building, till it comes down within ten feet of the ground, and thence carry it off to your ground rod; near to which should be planted a post, to support the iron conductor above the heads of people walking under it.

If the building be large and long, as an hundred feet or upwards, it may not be amiss to erect a pointed rod at each end, and form a communication by an iron rod between them. If there be a well near the house, so that you can by such a rod form a communication from your top rod to the water, it is rather better to do so than to use the ground rod above mentioned. It may also be proper to paint the iron, to render it more durable by preserving it from rust.

A building thus guarded, will not be damaged by lightning, nor any person or thing therein killed, hurt, or set on fire. For, either the explosion will be prevented by the operation of the point; or, if not prevented, then the whole quantity of lightning exploded near the house, whether passing from the cloud to the earth, or from the earth to the cloud, will be conveyed

in the rods. And, though the iron be crooked round the corner of the building, or make ever so many turns between the upper and lower rod, the lightning will follow it, and be guided by it, without affecting the building. I omit the philosophical reasons and experiments on which this practice is founded; for they are many, and would make a book. Besides they are already known to most of the learned throughout Europe. In the American British colonies, many houses have been, since the year 1752, guarded by these principles. Three facts have only come to my knowledge of the effects of lightning on such houses.*

If I have not been explicit enough in my directions, I shall, on the least intimation, endeavour to supply the defect.

I am, &c.

B. FRANKLIN.

FROM DAVID HUME TO B. FRANKLIN.

Remarks on the Mode of constructing Lightning-Rods. — Compliment to Dr. Franklin as a Philosopher and a Man of Letters.

Edinburgh, 10 May, 1762.

DEAR SIR,

I have a great many thanks to give you for your goodness in remembering my request, and for the exact description, which you sent me of your method of preserving houses from thunder. I communicated it to

* The statement of the three facts is here omitted, as they are those relating to the houses of Mr. West in Philadelphia, and of Mr. Raven and Mr. Maine in South Carolina, which are described in Volume V. pp. 375, 395, 396. — EDITOR.

our Philosophical Society, as you gave me permission; and they desire me to tell you, that they claim it as their own, and intend to enrich with it the first collection, which they may publish. The established rule of our Society is, that, after a paper is read to them, it is delivered by them to some member, who is obliged, in a subsequent meeting, to read some paper of remarks upon it.

It was communicated to our friend, Mr. Russel; who is not very expeditious in finishing any undertaking; and he did not read his remarks, till the last week, which is the reason, why I have been so late in acknowledging your favor. Mr. Russel's remarks, besides the just praises of your invention, contained only two proposals for improving it. One was, that in houses, where the rain-water is carried off the roof by a lead pipe, this metallic body might be employed as a conductor to the electric fire, and save the expense of a new apparatus. Another was, that the wire might be carried down to the foundation of the house, and be thence conveyed below ground to the requisite distance, which would better secure it against accidents. I thought it proper to convey to you these two ideas of so ingenious a man, that you might adopt them, if they appear to you well founded.

I am very sorry, that you intend soon to leave our hemisphere. America has sent us many good things, gold, silver, sugar, tobacco, indigo, &c.; but you are the first philosopher, and indeed the first great man of letters for whom we are beholden to her. It is our own fault, that we have not kept him; whence it appears, that we do not agree with Solomon, that wisdom is above gold; for we take care never to send back an ounce of the latter, which we once lay our fingers upon.

I saw yesterday our friend Sir Alexander Dick, who desired me to present his compliments to you. We are all very unwilling to think of your settling in America, and that there is some chance for our never seeing you again; but no one regrets it more than does,

Dear Sir,

Your most affectionate humble servant,

DAVID HUME.

TO JOHN BAPTIST BECCARIA.

Describing a new Musical Instrument composed of Glasses.

London, 13 July, 1762.

REVEREND SIR,

I once promised myself the pleasure of seeing you at Turin; but, as that is not now likely to happen, being just about returning to my native country, America, I sit down to take leave of you (among others of my European friends that I cannot see) by writing.

I thank you for the honorable mention you have so frequently made of me in your letters to Mr. Collinson and others, for the generous defence you undertook and executed with so much success, of my electrical opinions; and for the valuable present you have made me of your new work, from which I have received great information and pleasure. I wish I could in return entertain you with any thing new of mine on that subject; but I have not lately pursued it. Nor do I know of any one here, that is at present much engaged in it.

Perhaps, however, it may be agreeable to you, as you live in a musical country, to have an account of

the new instrument lately added here to the great number that charming science was before possessed of. As it is an instrument that seems peculiarly adapted to Italian music, especially that of the soft and plaintive kind, I will endeavour to give you such a description of it, and of the manner of constructing it, that you or any of your friends may be enabled to imitate it, if you incline so to do, without being at the expense and trouble of the many experiments I have made in endeavouring to bring it to its present perfection.

You have doubtless heard the sweet tone that is drawn from a drinking-glass by passing a wet finger round its brim. One Mr. Puckeridge, a gentleman from Ireland, was the first who thought of playing tunes, formed of these tones. He collected a number of glasses of different sizes, fixed them near each other on a table, and tuned them by putting into them water more or less, as each note required. The tones were brought out by passing his fingers round their brims. He was unfortunately burned here, with his instrument, in a fire which consumed the house he lived in. Mr. E. Delaval, a most ingenious member of our Royal Society, made one in imitation of it, with a better choice and form of glasses, which was the first I saw or heard. Being charmed by the sweetness of its tones, and the music he produced from it, I wished only to see the glasses disposed in a more convenient form, and brought together in a narrower compass, so as to admit of a greater number of tones, and all within reach of hand to a person sitting before the instrument, which I accomplished, after various intermediate trials and less commodious forms, both of glasses and construction, in the following manner.

The glasses are blown as near as possible in the form of hemispheres, having each an open neck or

socket in the middle. (See Plate X. Figure 1.) The thickness of the glass near the brim about a tenth of an inch, or hardly quite so much, but thicker as it comes nearer the neck, which in the largest glasses is about an inch deep, and an inch and half wide within, these dimensions lessening, as the glasses themselves diminish in size, except that the neck of the smallest ought not to be shorter than half an inch. The largest glass is nine inches diameter, and the smallest three inches. Between these two are twenty-three different sizes, differing from each other a quarter of an inch in diameter. To make a single instrument there should be at least six glasses blown of each size; and out of this number one may probably pick thirty-seven glasses (which are sufficient for three octaves with all the semitones) that will be each either the note one wants or a little sharper than that note, and all fitting so well into each other as to taper pretty regularly from the largest to the smallest. It is true there are not thirty-seven sizes, but it often happens that two of the same size differ a note or half note in tone, by reason of a difference in thickness, and these may be placed one in the other without sensibly hurting the regularity of the taper form.

The glasses being chosen, and every one marked with a diamond the note you intend it for, they are to be tuned by diminishing the thickness of those that are too sharp. This is done by grinding them round from the neck towards the brim, the breadth of one or two inches, as may be required; often trying the glass by a well-tuned harpsichord, comparing the tone drawn from the glass by your finger, with the note you want, as sounded by that string of the harpsichord. When you come nearer the matter, be careful to wipe the glass clean and dry before each trial, because the tone

is something flatter when the glass is wet, than it will be when dry; and, grinding a very little between each trial, you will thereby tune to great exactness. The more care is necessary in this, because, if you go below your required tone, there is no sharpening it again but by grinding somewhat off the brim, which will afterwards require polishing, and thus increase the trouble.

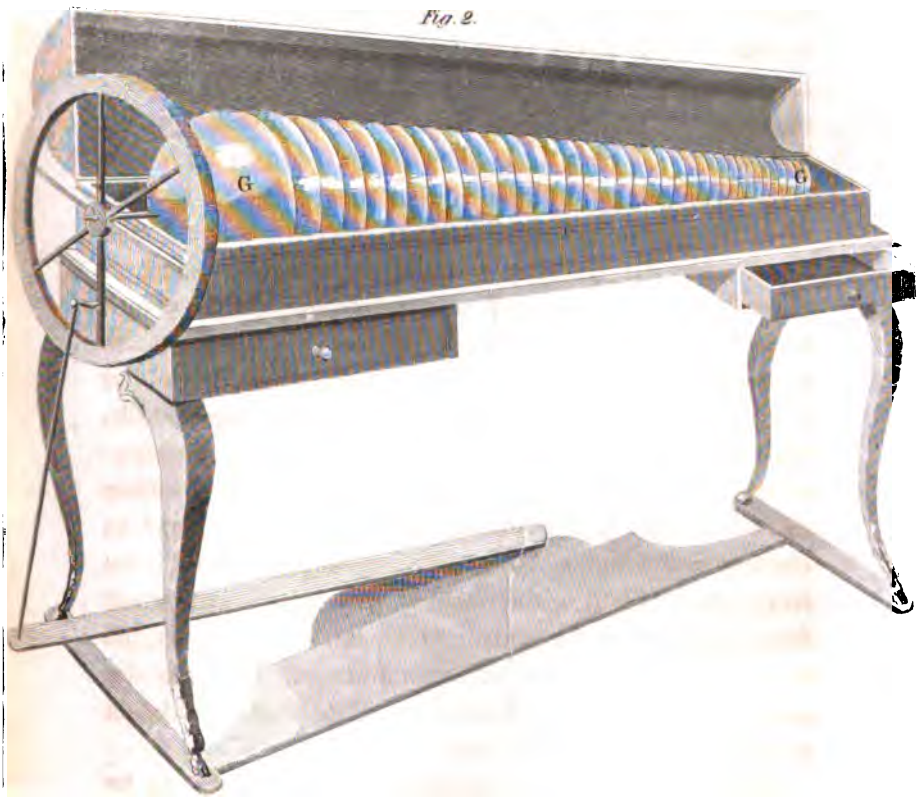
The glasses being thus tuned, you are to be provided with a case for them, and a spindle on which they are to be fixed. (See Plate X. Figure 2.) My case is about three feet long, eleven inches every way wide within at the biggest end, and five inches at the smallest end; for it tapers all the way, to adapt it better to the conical figure of the set of glasses. This case opens in the middle of its height, and the upper part turns up by hinges fixed behind. The spindle, which is of hard iron, lies horizontally from end to end of the box within, exactly in the middle, and is made to turn on brass gudgeons at each end. It is round, an inch diameter at the thickest end, and tapering to a quarter of an inch at the smallest. A square shank comes from its thickest end through the box, on which shank a wheel is fixed by a screw. This wheel serves as a fly to make the motion equable, when the spindle, with the glasses, is turned by the foot like a spinning-wheel. My wheel is of mahogany, eighteen inches diameter, and pretty thick, so as to conceal near its circumference about twenty-five pounds of lead. An ivory pin is fixed in the face of this wheel, and about four inches from the axis. Over the neck of this pin is put the loop of the string that comes up from the movable step to give it motion. The case stands on a neat frame with four legs.

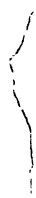
To fix the glasses on the spindle, a cork is first to be fitted in each neck pretty tight, and projecting a little

ARMONICA.

Fig. 1.

Fig. 2.





without the neck, that the neck of one may not touch the inside of another when put together, for that would make a jarring. These corks are to be perforated with holes of different diameters, so as to suit that part of the spindle on which they are to be fixed. When a glass is put on, by holding it stiffly between both hands, while another turns the spindle, it may be gradually brought to its place. But care must be taken that the hole be not too small, lest, in forcing it up, the neck should split; nor too large, lest the glass, not being firmly fixed, should turn or move on the spindle, so as to touch and jar against its neighbouring glass. The glasses thus are placed one in another, the largest on the biggest end of the spindle which is to the left hand; the neck of this glass is towards the wheel, and the next goes into it in the same position, only about an inch of its brim appearing beyond the brim of the first; thus proceeding, every glass when fixed shows about an inch of its brim (or three quarters of an inch, or half an inch, as they grow smaller) beyond the brim of the glass that contains it; and it is from these exposed parts of each glass that the tone is drawn, by laying a finger upon one of them as the spindle and glasses turn round.

My largest glass is G, a little below the reach of a common voice, and my highest G, including three complete octaves. To distinguish the glasses the more readily to the eye, I have painted the apparent parts of the glasses within side, every semitone white, and the other notes of the octave with the seven prismatic colors, *viz.* C, red; D, orange; E, yellow; F, green; G, blue; A, indigo; B, purple; and C, red again; so that glasses of the same color (the white excepted) are always octaves to each other.

This instrument is played upon, by sitting before the

middle of the set of glasses as before the keys of a harpsichord, turning them with the foot, and wetting them now and then with a spunge and clean water. The fingers should be first a little soaked in water, and quite free from all greasiness; a little fine chalk upon them is sometimes useful, to make them catch the glass and bring out the tone more readily. Both hands are used, by which means different parts are played together. Observe, that the tones are best drawn out when the glasses turn *from* the ends of the fingers, not when they turn *to* them.

The advantages of this instrument are, that its tones are incomparably sweet beyond those of any other; that they may be swelled and softened at pleasure by stronger or weaker pressures of the finger, and continued to any length; and that the instrument, being once well tuned, never again wants tuning.

In honor of your musical language, I have borrowed from it the name of this instrument, calling it the Armonica.*

With great esteem and respect, I am, &c.

B. FRANKLIN.

TO OLIVER NEAVE.

Respecting the best Mediums for conveying Sound.

20 July, 1762.

DEAR SIR,

I have perused your paper on sound, and would freely mention to you, as you desire it, every thing that

* Some other particulars respecting the *Armonica* may be found in a letter to M. Dubourg, under the date of December 8th, 1772.—EDITOR.

appeared to me to need correction; but nothing of that kind occurs to me, unless it be, where you speak of the air as "the *best* medium for conveying sound." Perhaps this is speaking rather too positively, if there be, as I think there are, some other mediums that will convey it farther and more readily. It is a well-known experiment, that the scratching of a pin at one end of a long piece of timber, may be heard by an ear applied near the other end, though it could not be heard at the same distance through the air. And, two stones being struck smartly together under water, the stroke may be heard at a greater distance by an ear also placed under water, than it can be heard through the air. I think I have heard it near a mile; how much farther it may be heard I know not; but suppose a great deal farther, because the sound did not seem faint, as if at a distance, like distant sounds through air, but smart and strong, and as if present just at the ear. I wish you would repeat these experiments now you are upon the subject, and add your own observations. And, if you were to repeat, with your naturally exact attention and observation, the common experiment of the bell in the exhausted receiver, possibly something new may occur to you, in considering,

1. Whether the experiment is not ambiguous; that is, whether the gradual exhausting of the air, as it creates an increasing difference of pressure on the outside, may not occasion in the glass a difficulty of vibrating, that renders it less fit to communicate to the air without, the vibrations that strike it from within; and the diminution of the sound arise from this cause, rather than from the diminution of the air?

2. Whether, as the particles of air themselves are at a distance from each other, there must not be some medium between them, proper for conveying

sound, since otherwise it would stop at the first particle?

3. Whether the great difference we experience in hearing sounds at a distance, when the wind blows towards us from the sonorous body, or towards that from us, can be well accounted for by adding to or subtracting from the swiftness of sound, the degree of swiftness that is in the wind at the time? The latter is so small in proportion, that it seems as if it could scarce produce any sensible effect, and yet the difference is very great. Does not this give some hint, as if there might be a subtile fluid, the conductor of sound, which moves at different times in different directions over the surface of the earth, and whose motion may perhaps be much swifter than that of the air in our strongest winds; and that, in passing through air, it may communicate that motion to the air which we call wind, though a motion in no degree so swift as its own?

4. It is somewhere related, that a pistol, fired on the top of an exceeding high mountain, made a noise like thunder in the valleys below. Perhaps this fact is not exactly related; but, if it is, would not one imagine from it, that the rarer the air, the greater sound might be produced in it from the same cause?

5. Those balls of fire which are sometimes seen passing over a country, computed by philosophers to be often thirty miles high at least, sometimes burst at that height; the air must be exceeding rare there, and yet the explosion produces a sound that is heard at that distance, and for seventy miles round on the surface of the earth, so violent too as to shake buildings, and give an apprehension of an earthquake. Does not this look as if a rare atmosphere, almost a vacuum, was no bad conductor of sound?

I have not made up my own mind on these points, and only mention them for your consideration, knowing that every subject is the better for your handling it.

With the greatest esteem, I am, &c.

B. FRANKLIN.

TO JOHN PRINGLE, IN LONDON.

Relating a curious Instance of the Effect of Oil on Water.

Philadelphia, 1 December, 1762.

SIR,

During our passage to Madeira, the weather being warm, and the cabin windows constantly open for the benefit of the air, the candles at night flared and run very much, which was an inconvenience. At Madeira, we got oil to burn, and with a common glass tumbler or beaker, slung in wire, and suspended to the ceiling of the cabin, and a little wire hoop for the wick, furnished with corks to float on the oil, I made an Italian lamp, that gave us very good light all over the table. The glass at bottom contained water to about one third of its height; another third was taken up with oil; the rest was left empty that the sides of the glass might protect the flame from the wind. There is nothing remarkable in all this; but what follows is particular. At supper, looking on the lamp, I remarked, that though the surface of the oil was perfectly tranquil, and duly preserved its position and distance with regard to the brim of the glass, the water under the oil was in great commotion, rising and falling in irregular waves, which continued during the whole evening. The lamp was kept burning as a watch-light all night, till the oil was

spent, and the water only remained. In the morning I observed, that though the motion of the ship continued the same, the water was now quiet, and its surface as tranquil as that of the oil had been the evening before. At night again, when oil was put upon it, the water resumed its irregular motions, rising in high waves almost to the surface of the oil, but without disturbing the smooth level of that surface. And this was repeated every day during the voyage.

Since my arrival in America, I have repeated the experiment frequently thus. I have put a pack-thread round a tumbler, with strings of the same, from each side, meeting above it in a knot at about a foot distance from the top of the tumbler. Then putting in as much water as would fill about one third part of the tumbler, I lifted it up by the knot, and swung it to and fro in the air; when the water appeared to keep its place in the tumbler as steadily as if it had been ice. But pouring gently in upon the water about as much oil, and then again swinging it in the air as before, the tranquillity before possessed by the water was transferred to the surface of the oil, and the water under it was agitated with the same commotions as at sea.

I have shown this experiment to a number of ingenious persons. Those who are but slightly acquainted with the principles of hydrostatics, &c. are apt to fancy immediately that they understand it, and readily attempt to explain it; but their explanations have been different, and to me not very intelligible. Others, more deeply skilled in those principles, seem to wonder at it, and promise to consider it. And I think it is worth considering; for a new appearance, if it cannot be explained by our old principles, may afford us new ones, of use perhaps in explaining some other obscure parts of natural knowledge. I am, &c.

B. FRANKLIN.

*Congelation of Quicksilver. — Cold produced by Evaporation.**

Perth Amboy, 26 February, 1763.

THE most remarkable discovery that has been made within these three years is, that quicksilver is in reality a melted metal, with this character only, that of all others it requires the least heat to melt it. The Academy of Sciences at Petersburg have found, that by dipping a mercurial thermometer into repeated cooling mixtures, and so taking from the mercury the heat that was in it, they have brought it down some hundred degrees (the exact number I cannot remember) below the freezing point, when the mercury became solid and would sink no longer; and then the glass being broke, it came out in the form of a silver bullet adhering to a wire, which was the slender part that had been in the tube. Upon trial it was found malleable and was hammered out to the bigness of a half-crown, but soon after on receiving a small degree of warmth it returned gradually to its fluid state again. This experiment was repeated by several members of that Academy two winters successively, and an authentic account of it transmitted to our Royal Society.

I suppose you have seen, in the second volume of the new Philosophical Essays of the Edinburgh Society, an account of some experiments to produce cold by evaporation made by Dr. Cullen, who mentions the like having been before made at Petersburg. I think it is but lately, that our European philosophers have known or acknowledged any thing of such a power in

* This is a fragment of a letter in the handwriting of Franklin, a part of which only has been found. It is not known to whom it was written. — EDITOR.

nature. But I find it has been long known in the east. Bernier, in the account of his travels into India, written above a hundred years since, mentions the custom of travellers carrying their water in flasks covered with wet wrappers, and hung to the pommels of their saddles, so as that the wind might act upon them, and so cool the water. I have also seen a kind of jar for cooling water, made of potter's earth glazed, and so porous that the water gradually oozed through to the surface, supplying water just sufficient for a constant evaporation. I tried it and found the water within much cooler in a few hours. This jar was brought from Egypt.

FROM JOHN CANTON TO B. FRANKLIN.*

The Melting of Metals by Lightning not a cold Fusion. — Compressibility of Water and other Fluids.

London, 29 June, 1764.

DEAR SIR,

Your favor of the 14th of March came to my hands the 15th of May last, and gave me great pleasure. The first experiment of Mr. Kinnersley's, which you mention, is, as you observe, a beautiful one to see; and,

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* Mr. Canton was a very ingenious experimental philosopher, and for many years an active and valuable member of the Royal Society. His papers in the Philosophical Transactions, and other publications, contributed much to the improvement of science. His fondness for electricity was the cause of an intimate friendship between him and Dr. Franklin, which continued till the death of Mr. Canton, on the 22d of March, 1772, in the fifty-fourth year of his age. His most important discovery was that of the compressibility of water, in opposition to the received opinion formed on the celebrated Florentine experiment. For this discovery he was honored by the Royal Society with the Copley Medal, in November, 1765. — EDITOR.

I think, fully proves that the fusion of metals by lightning is not a cold fusion. I have myself several times melted small brass wire by a stroke from your case of bottles, which left a mark where it lay upon the table, and some balls of twice or three times its diameter near the mark, but no part of the wire could be found. At the time of the stroke a great number of sparks, like those from a flint and steel, fly upward and laterally from the place where the wire was laid, and lose their light in the daytime, at the distance of about two or three inches. The diameter of a piece of Mr. Kinnersley's wire, which you were so kind as to send me with the balls, I found to be one part in one hundred and eighty-two of an inch; mine was but one part in three hundred and thirty.

The second of Mr. Kinnersley's experiments, which you relate, and which seems to be a very extraordinary one, I have several times endeavoured to make, but without success. The air with you must certainly be much drier than in England; for I have never observed the enclosed pith-balls to separate by the electrized air of a room, without having first heated the phial, notwithstanding which, they always came together in the phial, before the outward air had lost its electricity, as appears by their separating again when taken out of it. I once electrified the air of my largest room to a considerable degree, and by opening the windows and doors suffered the wind to blow through for about five minutes. I then shut them and examined the air in the room, but found no sign of electricity remaining. This air I electrified to about the same degree as before, and, leaving it confined, it retained a sensible degree of its electricity for more than three quarters of an hour. Hence I entirely agree with you, that the glass in Mr. Kinnersley's

experiment received some degree of electricity from the electrized air, and so kept the balls separated after that air was blown away.

I have put your ingenious friend Mr. Bowdoin's telescope into Mr. Nairne's hands, who is making a pedestal for it, which I think will be an improvement of that which Mr. Bowdoin has described in his last letter to me, which you saw. You may depend on my taking all possible care to get it well executed and soon. I find the fitting Dollond's micrometer to the telescope is impracticable.

Since the publication of a short paper in the Transactions, which contains an account of experiments to prove that water is not incompressible, I have discovered a remarkable property belonging to that fluid, which is new to me, though perhaps it may not be so to you. The property I mean is, its being less compressible in summer than in winter. This is contrary to what I find in spirit of wine and oil of olives, which are (as one would expect water to be) more compressible when expanded by heat, and less so when contracted by cold. For, when Fahrenheit's thermometer is at thirty-four degrees and the barometer at twenty-nine and a half, water is compressed by the weight of the atmosphere forty-nine parts in a million of its whole bulk, and spirit of wine sixty of the same parts. When the thermometer is at fifty degrees, water is compressed forty-six parts and spirit of wine sixty-six parts in a million by the same weight; and, when the thermometer is at sixty-four degrees, this weight will compress water no more than forty-four parts, but it will compress spirit of wine seventy-one of these parts.

As I am not able at present to account for this difference in the compressibility of water myself, I should be very glad to have your thoughts upon it.

The compression by the weight of the atmosphere, and the specific gravity, of the following fluids (which are all that I have yet tried) are set down as they were found in a temperate degree of heat, and when the barometer was at a mean height.

	Millionth parts.	Specific gravity.
Compression of spirit of wine	66	846
" oil of olives	48	918
" rain water	46	1,000
" sea water	40	1,028
" mercury	3	13,595

You will easily perceive that the compressions of these fluids by the same weight are not in the inverse ratio of their densities, or specific gravities, as might be expected. The compression of spirit of wine, for instance, being compared with that of rain water, is *greater* than in this proportion it ought to be, and the compression of sea water is *less*.

Mr. Price, Mr. Rose, Mr. Cooper, and the rest of the club, desire their most respectful compliments to you, and very much regret, as I do myself, your leaving England.

I am, with the most sincere regard,

Dear Sir, &c.

JOHN CANTON.

FROM EZRA STILES TO B. FRANKLIN.*

Thermometrical Experiments.

Newport, 20 February, 1765.

DEAR SIR,

If I ask too great a favor of you, to forward the enclosed letter to M. Lomonosow at Petersburg, I leave it entirely with you to suppress it. I have taken the liberty as you see, of asking an answer through your hands. If I make too free a use of your name and friendship, you have it in your power to prevent the abuse. At least, however, give me leave to ask from yourself an account of the discoveries of the Polar voyage, if such an one should be effected. I suppose your Petersburg correspondence is with Æpinus and Braunius.

If the Baltic voyages should continue to be prosecuted from America as they are begun, I should be glad of an epistolary connexion at Petersburg. Your residence in London, and even in the world, will not probably continue many years. I had thought to have availed myself of your friendship so far as to have asked your introduction to a correspondence in the philosophic way with some gentleman in London, but this I leave also to your humanity.

When you read the enclosed, you may consider it addressed to yourself, as well as to M. Lomonosow, particularly as to the thermometrical observations here of 1764, which your beneficence has enabled me to make. I have published a request in the prints, that gentlemen of curiosity would furnish themselves with barometers and thermometers, and publish like observations in the respective provinces, that we may par-

* Mr. Stiles was at this time a clergyman at Newport, Rhode Island. He was afterwards President of Yale College. — EDITOR.

ticularly be enabled to collect for 1766, annual accounts of the mercurial altitudes in several parts of each of the sixteen continental provinces, for four of which we have them already. But I fear this will fail of success chiefly for want of thermometers, unless the Royal Society should condescend to distribute them over America as they did over Europe and Asia about thirty years ago. Twenty thermometers judiciously distributed, besides those we have here already would answer the end.

Upon reading in the "Annual Register" of 1762, an account of the congelation of mercury, by M. Braunius, with artificial cold in Petersburg, in December, 1759, I ventured yesterday on some of his experiments of refrigeration with common salt, aquafortis, and spirit *sal marin.*, affused upon snow, though with no view of pursuing them to the fixation of mercury, lest I should break my thermometer. From Braunius I expected to find the temperature of the snow and the external air the same; but I found it otherwise. Yesterday, from ten in the morning to two in the afternoon, the thermometer in open air abroad was from thirty-seven to forty-three and the snow dissolving apace; during this time for four hours I repeatedly set the tube, or rather the bulb, in snow, covered with it to ten, to twenty, thirty, forty degrees, when on every trial it fell to thirty-two, and there became stationary. And when by artificial cold I had reduced it far below the freezing point, upon removing the tube to pure, fresh, unmixed snow, the mercury constantly rose, and became stationary at thirty-two; yet, removing the snow, it ascended to thirty-seven and beyond. Bringing the thermometer into the house, it rose to fifty; when the bulb being placed in a glass of snow, brought from abroad, the mercury instantly descended from fifty and above to thirty-two, and there was stationary. From thence

I concluded, that, although, below the freezing point, both snow and air might be of the same temperature, yet, when the natural temperature of the air is above freezing, it differs from that of snow, which in this case is always stationary at or about thirty-two.

This morning, the mercury being down to twenty-five abroad, I applied snow to the bulb expecting no alteration, yet it fell half a degree; and at noon there was nearly a degree's difference, the snow a degree colder than the air, though the air held the cold of twenty-eight and a half. But perhaps, after twenty or thirty degrees descent below the freezing point, there may be a coincidence of temperature, as it happened at the Braunian experiment of congealing mercury when he found both snow and air at two hundred and ten of Delisle corresponding to forty below of Fahrenheit, or seventy-two below his freezing point. This little occurrence is new to me, who see few other recent discoveries than what are exhibited in the Magazines. Had we duly the periodical publications of the several Academies of Sciences in Europe, or those of the London Royal Society only (of which we have here only the first ten volumes abridged), I might perhaps save you the trouble of this account.

We have had a severe winter in New England. January 27th, at sunrise, my thermometer stood at five below zero. I am told the same morning at Boston it was nine below. I hope you will not forget to recommend that ingenious and learned gentleman, Professor Winthrop, to the honors of the Royal Society, now that you are in London.*

I am, Sir,

Your most obedient servant,

EZRA STILES.

* Professor Winthrop was chosen a member of the Royal Society not long after the date of this letter. He was proposed by Dr. Franklin, as

TO LORD KAMES, AT EDINBURGH.

On the Harmony and Melody of the old Scotch Tunes.

London, 2 June, 1765.

In my passage to America I read your excellent work, the *Elements of Criticism*, in which I found great entertainment. I only wished you had examined more fully the subject of music, and demonstrated that the pleasure artists feel in hearing much of that composed in the modern taste, is not the natural pleasure arising from melody or harmony of sounds, but of the same kind with the pleasure we feel on seeing the surprising feats of tumblers and rope-dancers, who execute difficult things. For my part I take this to be really the case, and suppose it the reason why those who are unpractised in music, and therefore unacquainted with

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appears by the following recommendation, or certificate, transcribed from a paper in his handwriting.

"John Winthrop, Professor of Mathematics and Natural Philosophy in Harvard College at Cambridge in New England, being desirous of the honor of becoming a member of the Royal Society, we the subscribers, to whom he is known personally, or by his ingenious writings, communicated from time to time to the Society, do recommend him as worthy of that honor."

Dr. Franklin says, in one of his letters, that it was not usual at that time to choose persons into the Royal Society, who had not requested to be admitted; and it was moreover necessary, that a "recommendatory certificate should be presented to the Society in favor of the candidate, signed by at least three of the members," expressing his desire to be chosen and his qualifications. There were some exceptions, however; for Dr. Franklin himself was elected without any previous application on his part, or any knowledge that he was to be proposed. The certificate in his favor was signed by the Earl of Macclesfield, then President of the Society, Lord Parker, and Lord Willoughby.

An ingenious letter from Professor Winthrop to Dr. Franklin, concerning the effect of the aberration of light on the time of the transit of Venus over the sun, and another letter to him from Dr. Price on the same subject, are contained in the *Philosophical Transactions*, Vol. LX. pp. 858, 536. — Editor.

those difficulties, have little or no pleasure in hearing this music. Many pieces of it are mere compositions of tricks. I have sometimes, at a concert, attended by a common audience, placed myself so as to see all their faces, and observed no signs of pleasure in them during the performance of a great part that was admired by the performers themselves; while a plain old Scotch tune, which they disdained, and could scarcely be prevailed on to play, gave manifest and general delight.

Give me leave, on this occasion, to extend a little the sense of your position, that "melody and harmony are separately agreeable, and in union delightful," and to give it as my opinion, that the reason why the Scotch tunes have lived so long, and will probably live for ever (if they escape being stifled in modern affected ornament), is merely this, that they are really compositions of melody and harmony united, or rather that their melody is harmony. I mean the simple tunes sung by a single voice. As this will appear paradoxical, I must explain my meaning. In common acceptation, indeed, only an agreeable *succession* of sounds is called *melody*, and only the *coexistence* of agreeable sounds, *harmony*. But, since the memory is capable of retaining for some moments a perfect idea of the pitch of a past sound, so as to compare with it the pitch of a succeeding sound, and judge truly of their agreement or disagreement, there may and does arise from thence a sense of harmony between the present and past sounds, equally pleasing with that between two present sounds.

Now the construction of the old Scotch tunes is this, that almost every succeeding emphatical note is a third, a fifth, an octave, or in short some note that is in concord with the preceding note. Thirds are chiefly used, which are very pleasing concords. I use the word *emphatical* to distinguish those notes which have a

stress laid on them in singing the tune, from the lighter connecting notes, that serve merely, like grammar articles in common speech, to tack the whole together.

That we have a most perfect idea of a sound just past, I might appeal to all acquainted with music, who know how easy it is to repeat a sound in the same pitch with one just heard. In tuning an instrument, a good ear can as easily determine that two strings are in unison by sounding them separately, as by sounding them together; their disagreement is also as easily, I believe I may say more easily and better distinguished, when sounded separately; for when sounded together, though you know by the beating that one is higher than the other, you cannot tell which it is. I have ascribed to memory the ability of comparing the pitch of a present tone with that of one past. But, if there should be, as possibly there may be, something in the ear, similar to what we find in the eye, that ability would not be entirely owing to memory. Possibly the vibrations given to the auditory nerves by a particular sound may actually continue some time after the cause of those vibrations is past, and the agreement or disagreement of a subsequent sound become by comparison with them more discernible. For the impression made on the visual nerves by a luminous object will continue for twenty or thirty seconds. Sitting in a room, look earnestly at the middle of a window a little while when the day is bright, and then shut your eyes; the figure of the window will still remain in the eye, and so distinct that you may count the panes.

A remarkable circumstance attending this experiment, is, that the impression of forms is better retained than that of colors; for after the eyes are shut, when you first discern the image of the window, the panes appear dark, and the cross bars of the sashes, with the

window frames and walls, appear white or bright; but, if you still add to the darkness in the eyes by covering them with your hand, the reverse instantly takes place, the panes appear luminous and the cross bars dark. And by removing the hand they are again reversed. This I know not how to account for. Nor for the following; that, after looking long through green spectacles, the white paper of a book will on first taking them off appear to have a blush of red; and, after long looking through red glasses, a greenish cast; this seems to intimate a relation between green and red not yet explained.

Farther, when we consider by whom these ancient tunes were composed, and how they were first performed, we shall see that such harmonical successions of sounds was natural and even necessary in their construction. They were composed by the minstrels of those days to be played on the harp accompanied by the voice. The harp was strung with wire, which gives a sound of long continuance, and had no contrivance like that in the modern harpsichord, by which the sound of the preceding could be stopped, the moment a succeeding note began. To avoid actual discord, it was therefore necessary that the succeeding emphatic note should be a chord with the preceding, as their sounds must exist at the same time. Hence arose that beauty in those tunes that has so long pleased, and will please for ever, though men scarce know why. That they were originally composed for the harp, and of the most simple kind, I mean a harp without any half notes but those in the natural scale, and with no more than two octaves of strings, from C to C, I conjecture from another circumstance, which is, that not one of those tunes, really ancient, has a single artificial half note in it, and that in tunes where

it was most convenient for the voice to use the middle notes of the harp, and place the key in F, there the B, which if used should be a B flat, is always omitted, by passing over it with a third. The connoisseurs in modern music will say, I have no taste; but I cannot help adding, that I believe our ancestors, in hearing a good song, distinctly articulated, sung to one of those tunes, and accompanied by the harp, felt more real pleasure than is communicated by the generality of modern operas, exclusive of that arising from the scenery and dancing. Most tunes of late composition, not having this natural harmony united with their melody, have recourse to the artificial harmony of a bass, and other accompanying parts.* This support, in my opin-

* Rousseau, in his *Dictionnaire de Musique*, printed in 1768, appears to have similar sentiments of our modern harmony, viz.

"M. Rameau prétend que les dessus d'une certaine simplicité suggèrent naturellement lour basse, et qu'un homme ayant l'oreille juste et non exercée, entonnera naturellement cette basse. C'est-là un préjugé de musicien, démenti par toute expérience. Non seulement celui qui n'aura jamais entendu ni basse ni harmonie, ne trouvera, de lui-même, ni cette harmonie ni cette basse; mais elles lui déplairont, si on les lui fait entendre, et il aimera beaucoup mieux le simple unisson.

"Quand on songe que, de tous les peuples de la terre, qui tous ont une musique et un chant, les Européens sont les seuls qui aient une harmonie des accords, et qui trouvent ce mélange agréable; quand on songe que le monde a duré tant de siècles, sans que, de toutes les nations qui ont cultivé les beaux arts, aucune ait connu cette harmonie; qu'aucun animal, qu'aucun oiseau, qu'aucun être dans la nature ne produit d'autre accord que l'unisson, ni d'autre musique que la mélodie; que les langues orientales, si sonores, si musicales; qui les oreilles Grecques, si délicates, si sensibles, exercées avec tant d'art, n'ont jamais guidé ces peuples voluptueux et passionnés vers notre harmonie; que, sans elle, leur musique avoit des effets si prodigieux: qu'avec elle la nôtre en a de si foibles: qu'enfin il étoit réservé à des peuples du Nord, dont les organes durs et grossiers sont plus touchés de l'éclat et du bruit des voix, que de la douceur des accens, et de la mélodie des inflexions, de faire cette grande découverte, et de la donner pour principe à toutes les règles de l'art; quand, dis-je, on fait attention à tout cela, il est bien difficile de ne pas soupçonner que toute notre harmonie n'est qu'une invention gothique et barbare, dont nous ne nous fussions jamais aviséa, si nous fus-

ion, the old tunes do not need, and are rather confused than aided by it. Whoever has heard James Oswald play them on his violoncello, will be less inclined to dispute this with me. I have more than once seen tears of pleasure in the eyes of his auditors; and yet, I think, even *his* playing those tunes would please more, if he gave them less modern ornament.

I am, &c.

B. FRANKLIN.*

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sions été plus sensibles aux véritables beautés de l'art, et à la musique vraiment naturelle."

* Mr. Tytler, in his *Life of Lord Kames*, (Vol. II. p. 31, 2d ed.) makes the following remarks on the above letter. "This notion of Dr. Franklin's, respecting what may be called the Ideal Harmony of the Scottish melodies, is extremely acute, and is marked by that ingenious simplicity in the thought, which is the characteristic of a truly philosophic mind. In supplement to his observation, that the past sound, being retained by the memory, forms a concord with the present sound, it may perhaps be added, that, the tympanum of the ear continuing to vibrate for some little time after it is struck by any musical note, the succeeding note will be either agreeable or disagreeable, as it accords, or is in discordance with the existing vibration. Now a succession of notes by thirds and fifths will always find the tympanum in concord, and the last vibration harmonizing with the succeeding. This notion accounts completely for the effect of the Scottish melodies, in giving pleasure alike to an intelligent judge of music, and to a person of uncultivated taste, provided he have a good musical ear; for the pleasure arising from a succession of sounds, in the regular intervals of thirds and fifths, and likewise that arising from their concord, is founded in nature, and in the mechanical structure of the organs of hearing, and is altogether independent of custom or acquired taste. A Scottish air will therefore be grateful alike to the ear of a Greenlander, a Japanese, and a native of Italy; if possessed of the musical sense, they will all equally understand and relish it, for it speaks an universal language." — EDITOR.

TO PETER FRANKLIN, AT NEWPORT.

On the Defects of Modern Music.

[No date.]

DEAR BROTHER,

I like your ballad, and think it well adapted for your purpose of discountenancing expensive foppery, and encouraging industry and frugality. If you can get it generally sung in your country, it may probably have a good deal of the effect you hope and expect from it. But, as you aimed at making it general, I wonder you chose so uncommon a measure in poetry, that none of the tunes in common use will suit it. Had you fitted it to an old one, well known, it must have spread much faster than I doubt it will do from the best new tune we can get composed for it. I think too, that if you had given it to some country girl in the heart of the Massachusetts, who has never heard any other than psalm tunes, or *Chevy Chace*, the *Children in the Wood*, the *Spanish Lady*, and such old simple ditties, but has naturally a good ear, she might more probably have made a pleasing popular tune for you, than any of our masters here, and more proper for your purpose, which would best be answered, if every word could as it is sung be understood by all that hear it, and if the emphasis you intend for particular words could be given by the singer as well as by the reader; much of the force and impression of the song depending on those circumstances. I will however get it as well done for you as I can.

Do not imagine that I mean to depreciate the skill of our composers of music here; they are admirable at pleasing *practised* ears, and know how to delight *one another;* but, in composing for songs, the reigning

taste seems to be quite out of nature, or rather the reverse of nature, and yet, like a torrent, hurries them all away with it; one or two perhaps only excepted.

You, in the spirit of some ancient legislators, would influence the manners of your country by the united powers of poetry and music. By what I can learn of *their* songs, the music was simple, conformed itself to the usual pronunciation of words, as to measure, cadence or emphasis, &c., never disguised and confounded the language by making a long syllable short, or a short one long, when sung; their singing was only a more pleasing, because a melodious manner of speaking; it was capable of all the graces of prose oratory, while it added the pleasure of harmony. A modern song, on the contrary, neglects all the proprieties and beauties of common speech, and in their place introduces its *defects* and *absurdities* as so many graces. I am afraid you will hardly take my word for this, and therefore I must endeavour to support it by proof. Here is the first song I lay my hand on. It happens to be a composition of one of our greatest masters, the ever-famous Handel. It is not one of his juvenile performances, before his taste could be improved and formed; it appeared when his reputation was at the highest, is greatly admired by all his admirers, and is really excellent in its kind. It is called, "The additional favorite Song in Judas Maccabeus." Now I reckon among the defects and improprieties of common speech, the following, viz.

1. *Wrong placing the accent or emphasis*, by laying it on words of no importance, or on wrong syllables.

2. *Drawling;* or extending the sound of words or syllables beyond their natural length.

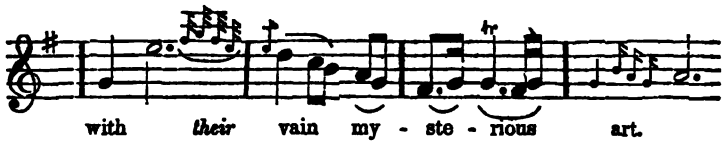
3. *Stuttering;* or making many syllables of one.

4. *Unintelligibleness;* the result of the three foregoing united.

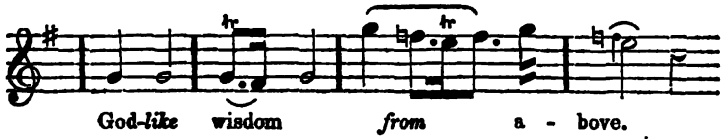
5. *Tautology;* and

6. *Screaming,* without cause.

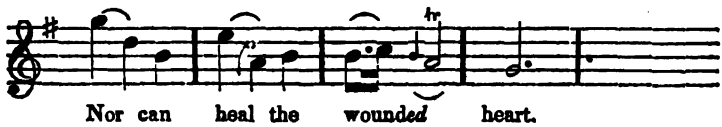
For the *wrong placing of the accent, or emphasis,* see it on the word *their* instead of being on the word *vain.*



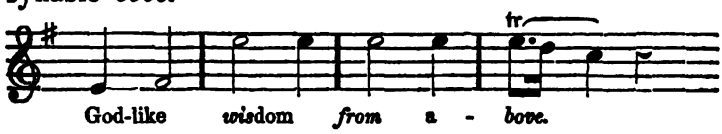
And on the word *from,* and the wrong syllable *like.*



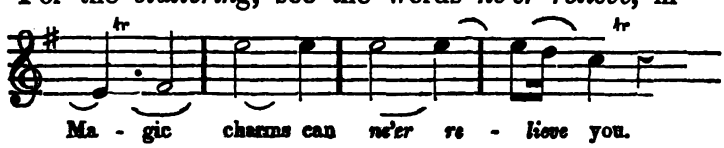
For the *drawling,* see the last syllable of the word *wounded.*



And in the syllable *wis,* and the word *from,* and syllable *bove.*



For the *stuttering,* see the words *ne'er relieve,* in

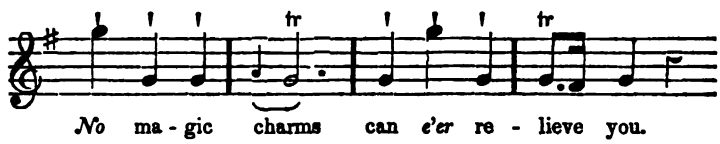


Here are four syllables made of one, and eight of three; but this is moderate. I have seen in another song, that I cannot now find, seventeen syllables made of three, and sixteen of one. The latter I remember was the word *charms;* viz. *cha, a, a, a, a, a, a, a, a, a, a, a, a, a, arms.* Stammering with a witness!

For the *unintelligibleness;* give this whole song to any taught singer, and let her sing it to any company that have never heard it; you shall find they will not understand three words in ten. It is therefore, that, at the oratorios and operas, one sees with books in their hands all those who desire to understand what they hear sung by even our best performers.

For the *tautology;* you have, *with their vain mysterious art,* twice repeated; *magic charms can ne'er relieve you,* three times. *Nor can heal the wounded heart,* three times. *Godlike wisdom from above,* twice; and, *this alone can ne'er deceive you,* two or three times. But this is reasonable when compared with *the Monster Polypheme, the Monster Polypheme,* a hundred times over and over, in his admired *Acis and Galatea.*

As to the *screaming;* perhaps I cannot find a fair instance in this song; but whoever has frequented our operas will remember many. And yet here methinks the words *no* and *e'er,* when sung to these notes, have a little of the air of *screaming,* and would actually be screamed by some singers.



I send you enclosed the song with its music at length. Read the words without the repetitions. Observe how few they are, and what a shower of notes attend them; you will then perhaps be inclined to think with me, that, though the words might be the principal part of an ancient song, they are of small importance in a modern one; they are in short only a *pretence for singing.*

I am, as ever,

Your affectionate brother,

B. FRANKLIN.

P. S. I might have mentioned *inarticulation* among the defects in common speech, that are assumed as beauties in modern singing. But, as that seems more the fault of the singer than of the composer, I omitted it in what related merely to the composition. The fine singer, in the present mode, stifles all the hard consonants, and polishes away all the rougher parts of words that serve to distinguish them one from another; so that you hear nothing but an admirable pipe, and understand no more of the song, than you would from its tune played on any other instrument. If ever it was the ambition of musicians to make instruments that should imitate the human voice, that ambition seems now reversed, the voice aiming to be like an instrument. Thus wigs were first made to imitate a good natural head of hair; but when they became fashionable, though in unnatural forms, we have seen natural hair dressed to look like wigs.

FROM JOSEPH PRIESTLEY TO B. FRANKLIN.

Relating to a History of Electricity. — Curious Experiments with an Electrified Chain.

Warrington, 21 September, 1766.

DEAR SIR,

I wrote to Mr. Price last post, in which I desired him to remind you of your promise to procure me Beccaria's work, which you said you thought you could do of M. Delaval. Fearing he might not see you soon, I write to desire you to get it for me if possible, without loss of time. Otherwise, I must reserve his experiments for an *Appendix*; for, by the references I meet with to them, I find my book absolutely must not come

abroad without them. I am in such haste, as we have already begun to print, and have done five sheets.* The whole work will make betwixt four hundred and five hundred pages quarto, and we shall not have done quite so soon as Christmas. For the same reason, I must beg you would also send me, as soon as you conveniently can, the two last numbers of the work which I left in the hands of Dr. Watson to be transmitted to you. I am now wholly employed in revising and correcting. I defer drawing up the account of my own experiments, till I have some more in pursuance of them and several others. In about a week I shall betake myself to experiments in good earnest; but I have no expectation of doing much more than I have done. Upon Mr. Price's letter I sent the mark that was actually made by a chain, when a discharge was sent through it. I have several times since I came home got *three* and almost always *two* concentric circles, upon the metal knobs with which I make discharges. If I verify your experiments on the *electrified cup* and *animal fluids*, may I publish them as yours in some proper place in my work? I shall soon go about them. Have you procured the list of books written on the subject of electricity, or the remainder of Wilkes's Treatise? Dr. Watson has sent me a curious tract of Johannes Franciscus Agua, which I am now digesting. In your note on one of my numbers you say, you question whether an æolipile will turn the same way, whether it draw in or throw out the water. I had tried it before I wrote the paragraph; you may depend upon the fact. I hope to find more of your excellent remarks upon the two numbers in your hands.

I am, dear Sir, &c.

JOSEPH PRIESTLEY.

* Priestley was now engaged in writing his *History of Electricity*, which was the work here alluded to. — EDITOR.

P. S. Since I wrote the letter, I made a discharge through a chain, that lay on this side of it. At the moment of the discharge, the whole appeared like a bright flame. From *a*, where the chain was returned, it was thrown back as far as *b*. I have tried the same several times since, laying one half of a chain parallel to the other, and marking exactly how far it reached upon the table; and always found the middle part pulled back about one inch and a half, as if a sudden jerk had been given to it. Indeed it was manifest, by comparing the link with the mark, that every link had moved a little. Must not that have been effected by the links repelling one another while the stroke was passing? Is not the paper really *burnt?* Was not the chain made superficially hot? And does not the electric shock pass chiefly over the surfaces of bodies? so that small bodies will be melted, because they have most surface in proportion to their bulk. The faint marks at a distance from the rest are not made by handling. They are just as the discharge left them. Indeed you will find they are not easily effaced. The wire of the chain is not so thick as the marks.*

TO GEORGE CROGHAN.

Conjecture as to Elephants being Natives of America.

London, 5 August, 1767.

SIR,

I return you many thanks for the box of elephants' tusks and grinders. They are extremely curious on

* On the face of the original letter, from which the above has been copied, the marks of the chain are still distinctly visible as here described, it being seventy-one years since the experiment was performed. The marks are of a yellowish green color. — EDITOR.

many accounts; no living elephants having been seen in any part of America by any of the Europeans settled there, or remembered in any tradition of the Indians. It is also puzzling to conceive what should have brought so many of them to die on the same spot; and that no such remains should be found in any other part of the continent, except in that very distant country, Peru, from whence some grinders of the same kind formerly brought, are now in the museum of the Royal Society. The tusks agree with those of the African and Asiatic elephant in being nearly of the same form and texture, and some of them, notwithstanding the length of time they must have lain, being still good ivory. But the grinders differ, being full of knobs, like the grinders of a carnivorous animal; when those of the elephant, who eats only vegetables, are almost smooth. But then we know of no other animal with tusks like an elephant, to whom such grinders might belong.

It is remarkable, that elephants now inhabit naturally only hot countries where there is no winter, and yet these remains are found in a winter country; and it is no uncommon thing to find elephants' tusks in Siberia, in great quantities, when their rivers overflow, and wash away the earth, though Siberia is still more a wintry country than that on the Ohio; which looks as if the earth had anciently been in another position, and the climates differently placed from what they are at present.

With great regard, I am, Sir,

Your most obedient humble servant,

B. FRANKLIN.*

* The bones referred to in this letter were presented by Dr. Franklin to the Royal Society. In the *Philosophical Transactions*, (Vol. LVII, p. 464,) they are described in a paper communicated by Mr. Peter Collin-

TO M. DALIBARD.

London, 31 January, 1768.

DEAR SIR,

I sent you some time since Priestley's *History of Electricity*, under the care of Mr. Molini, bookseller on the Quay des Augustins. I hope it got safe to Paris, and that you have reviewed it. I wish the reading of it may renew your taste for that branch of philosophy, which is already indebted to you as being the first of mankind, that had the courage to attempt drawing lightning from the clouds to be subjected to your experiments.*

In our return home, † we were detained a week at Calais by contrary winds and stormy weather, which was the more mortifying to me, when I reflected that I might have enjoyed Paris and my friends there all this time, and yet have been as soon at London.

As I became in arrear with my business by so long

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son. At the beginning of that paper, Mr. Collinson says; "George Croghan, who is a deputy of Sir William Johnson, the King's superintendent of Indian affairs in America, in the course of his navigation down the great river Ohio, after passing the Miami river, in the evening came near the place where the elephants' bones are found, about four miles southeast of the Ohio, and about six hundred miles distant from and below Pittsburg; from the nearest sea-coast at least seven hundred miles. Next morning he met with a large road, which the buffaloes had beaten, wide enough for two wagons to go abreast, leading straight into the great licking-place, to which the buffaloes and all the species of deer resort, at a certain season of the year, to lick the earth and water from salt springs, that are impregnated with nitrous particles." Mr. Collinson's account is accompanied with drawings of some of the bones. They were at first supposed to be the bones of elephants, but it was afterwards ascertained that they belonged to an extinct race of animals called by naturalists the Great Mastodon. — EDITOR.

* See, in Vol. V. p. 288, an account of M. Dalibard's experiments at Marly. — EDITOR.

† Dr. Franklin had recently returned to England from a tour in France. — EDITOR.

an absence, I have been necessarily much occupied since my return, and have therefore postponed from time to time (and so long that I am now ashamed of it) the purpose I had of writing soon to you, to express the sense I have of your kindness to me when a stranger at Paris, and of the many civilities I received from you there and from Mrs. Dalibard, which I assure you have made a lasting impression on my memory. I beg you will both of you accept my sincerest thanks and acknowledgments. The time I spent in Paris, and in the improving conversation and agreeable society of so many ingenious and learned men, seems now to me like a pleasing dream, from which I was only to be awakened by finding myself at London.

With the greatest esteem and best wishes for your health and happiness, I have the honor to be,

Dear Sir, &c.

B. FRANKLIN.

TO CADWALLADER EVANS.*

On the Colica Pictonum, and Pernicious Use of Lead in Distilleries.

London, 20 February, 1768.

DEAR SIR,

I wrote you a few lines by Captain Falconer, and sent you Dr. Watson's new piece of Experiments in Inoculation, which I hope will be agreeable to you.

In yours of November 20th, you mention the lead in the worms of stills as a probable cause of the dry belly-ache among punch-drinkers in our West Indies.

* A physician in Philadelphia, and member of the American Philosophical Society. — EDITOR.

I had before acquainted Dr. Baker with a fact of that kind, the general mischief done by the use of leaden worms, when rum-distilling was first practised in New England, which occasioned a severe law there against them; and he has mentioned it in the second part of his piece not yet published. I have long been of opinion, that that distemper proceeds always from a metallic cause only; observing that it affects, among tradesmen, those that use lead, however different their trades, —as glaziers, letter-founders, plumbers, potters, white-lead makers, and painters; (from the latter, it has been conjectured, it took its name *colica Pictonum*, by the mistake of a letter, and not from its being the disease of Poictou;) and, although the worms of stills ought to be of pure tin, they are often made of pewter, which has a great mixture in it of lead.

The Boston people, pretending to interfere with the manufactures of this country, make a great clamor here against America in general. I have therefore endeavoured to palliate matters a little in several public papers. It would, as you justly observe, give less umbrage if we meddled only with such manufactures as England does not attend to. That of linen might be carried on more or less in every family, and silk, I think, in most of the colonies. But there are many manufactures that we cannot carry on to advantage, though we were at entire liberty. And, after all, the true source of riches is husbandry. That is truly productive of new wealth; manufacturers only change forms, and, whatever value they give to the materials they work upon, they at the same time consume an equal value in provisions, &c. So that riches are not increased by manufacturing; the only advantage is, that provisions in the shape of manufactures are more easily carried for sale to foreign markets. And where they cannot be

easily carried to market, it is well to transform them for our own use as well as foreign sale. In families also, where the children and servants of families have some spare time, it is well to employ it in making something. I am, with thanks for your good wishes, dear Sir, yours, &c.

B. FRANKLIN.

TO LORD KAMES.

On Chimneys.

London, 28 February, 1768.

IT gave me great pleasure to see my dear good friend's name at the foot of a letter I received the other day, having been often uneasy at his long silence, blaming myself as the cause by my own previous backwardness and want of punctuality as a correspondent. I now suppose, (as in this he mentions nothing of it,) that a long letter I wrote him about this time twelvemonth, on the subject of the disputes with America, did miscarry, or that his answer to that letter miscarried, as I have never heard from him since I wrote that letter.

I have long been of an opinion similar to that you express, and think happiness consists more in small conveniences or pleasures that occur every day, than in great pieces of good fortune that happen but seldom to a man in the course of his life. Thus I reckon it among my felicities, that I can set my own razor, and shave myself perfectly well; in which I have a daily pleasure, and avoid the uneasiness one is sometimes obliged to suffer from the dirty fingers or bad breath of a slovenly barber.

I congratulate you on the purchase of a new house so much to your mind, and wish that you may long inhabit it with comfort. The inconvenience you mention of neighbouring smoke coming down the vents, is not owing to any bad construction of the vent down which it comes, and therefore not to be remedied by any change of form. It is merely the effect of a law of nature, whereby, whenever the outward air is warmer than the walls of the vent, the air included, being by those walls made colder, and of course denser and heavier than an equal column of the outward air, descends into the room, and in descending draws other air into the vent above to supply its place; which, being in its turn cooled and condensed by the cooler walls of the vent, descends also, and so a current downwards is continued during the continuance of such difference in temperament between the outward air and the walls of the vent.

When this difference is destroyed, by the outward air growing cooler, and the walls growing warmer, the current downwards ceases; and, when the outward air becomes still colder than the walls, the current changes and moves from below upwards, the warmer walls rarefying the air they include, and thereby making it so much lighter than a column of the outward air of equal height, that it is obliged to give way to the other's superior weight and rise, is succeeded by colder air, which being warmed and rarefied in its turn, rises also, and so the upward current is continued. In summer, when fires are not made in the chimneys, the current generally sets downward from nine or ten in the morning during all the heat of the day, till five or six in the afternoon, then begins to hesitate, and afterwards to set upwards during the night, continuing till about nine in the morning, then hesitating for some time be-

fore it again sets downwards for the day. This is the general course, with some occasional variation of hours, according to the length of days or changes of weather.

Now when the air of any vent is in this descending state, if the smoke issuing from a neighbouring vent happens to be carried over it by the wind, part will be drawn in and brought down into the room. The proper remedy then is, to close the opening of the chimney in the room by a board so fitted that little or no air can pass, whereby the currents abovementioned will be prevented. This board to remain during the summer, and when fires are not made in the chimney. Chimneys that have fires in them daily are not subject to this inconvenience, the walls of their vents being kept too warm to occasion any downward current during the hours between the going out of one fire and the kindling of another. And indeed, in summer, those vents that happen to go up close joined with the kitchen vent, are generally kept so warm by that as to be free from the downward current, and therefore free from what you call neighbour smoke.

The Philadelphia grate which you mention is a very good thing, if you could get one that is rightly made, and a workman skilful in putting them up. Those generally made and used here are much hurt by fancied improvements in their construction, and I cannot recommend them. As fuel with you is cheap and plenty, a saving in it is scarce an object. The sliding plates (of which I sent a model to Sir Alexander Dick) are, in my opinion, the most convenient for your purpose, as they keep a room sufficiently warm, are simple machines, easily fixed, and their management easily conceived and understood by servants.

I shall leave Europe with much greater regret, if I cannot first visit you and my other friends in Scotland.

I promise myself this happiness, but am not yet clear that I shall have time for it. Your kind invitation is extremely obliging.

With sincere esteem I am, my dear friend,

Yours most affectionately,

B. FRANKLIN.

TO SIR JOHN PRINGLE.

On the Difference of Navigation in Shoal and Deep Water.

Craven Street, 10 May, 1768.

SIR,

You may remember, that when we were travelling together in Holland, you remarked, that the *trackschuit* in one of the stages went slower than usual, and inquired of the boatman, what might be the reason; who answered, that it had been a dry season, and the water in the canal was low. On being again asked if it was so low as that the boat touched the muddy bottom; he said, no, not so low as that, but so low as to make it harder for the horse to draw the boat. We neither of us at first could conceive, that, if there was water enough for the boat to swim clear of the bottom, its being deeper would make any difference. But, as the man affirmed it seriously as a thing well known among them, and as the punctuality required in their stages was likely to make such difference, if any there were, more readily observed by them, than by other watermen who did not pass so regularly and constantly backwards and forwards in the same track, I began to apprehend there might be something in it, and attempted to account for it from this consideration, that the boat, in proceeding along the canal, must in every

boat's length of her course, move out of her way a body of water equal in bulk to the room her bottom took up in the water; that the water so moved must pass on each side of her and under her bottom to get behind her; that, if the passage under her bottom was straitened by the shallows, more of that water must pass by her sides, and with a swifter motion, which would retard her, as moving the contrary way; or that, the water becoming lower behind the boat than before, she was pressed back by the weight of its difference in height, and her motion retarded by having that weight constantly to overcome. But, as it is often lost time to attempt accounting for uncertain facts, I determined to make an experiment of this, when I should have convenient time and opportunity.

After our return to England, as often as I happened to be on the Thames, I inquired of our watermen whether they were sensible of any difference in rowing over shallow or deep water. I found them all agreeing in the fact, that there was a very great difference, but they differed widely in expressing the quantity of the difference; some supposing it was equal to a mile in six, others to a mile in three, &c. As I did not recollect to have met with any mention of this matter in our philosophical books, and conceiving, that, if the difference should really be great, it might be an object of consideration in the many projects now on foot for digging new navigable canals in this island, I lately put my design of making the experiment in execution, in the following manner.

I provided a trough of planed boards fourteen feet long, six inches wide, and six inches deep, in the clear, filled with water within half an inch of the edge, to represent a canal. I had a loose board of nearly the same length and breadth, that, being put into the water, might be sunk to any depth, and fixed by little wedges

where I would choose to have it stay, in order to make different depths of water, leaving the surface at the same height with regard to the sides of the trough. I had a little boat in form of a lighter or boat of burthen, six inches long, two inches and a quarter wide, and one inch and a quarter deep. When swimming, it drew one inch water. To give motion to the boat, I fixed one end of a long silk thread to its bow, just even with the water's edge, the other end passed over a well made brass pulley, of about an inch diameter, turning freely on a small axis; and a shilling was the weight. Then placing the boat at one end of the trough, the weight would draw it through the water to the other.

Not having a watch that shows seconds, in order to measure the time taken up by the boat in passing from end to end, I counted as fast as I could count to ten repeatedly, keeping an account of the number of tens on my fingers. And, as much as possible to correct any little inequalities in my counting, I repeated the experiment a number of times at each depth of water, that I might take the medium. And the following are the results.

	Water 1¼ inches deep.	2 inches.	4½ inches.
1st exp.	100	94	79
2	104	93	78
3	104	91	77
4	106	87	79
5	100	88	79
6	99	86	80
7	100	90	79
8	100	88	81
	813	717	632
	Medium 101	Medium 89	Medium 79

I made many other experiments, but the above are those in which I was most exact; and they serve sufficiently to show that the difference is considerable. Between the deepest and shallowest it appears to be somewhat more than one fifth. So that, supposing large canals and boats and depths of water to bear the same proportions, and that four men or horses would draw a boat in deep water four leagues in four hours, it would require five to draw the same boat in the same time as far in shallow water; or four would require five hours.

Whether this difference is of consequence enough to justify a greater expense in deepening canals, is a matter of calculation, which our ingenious engineers in that way will readily determine.

I am, &c.

B. FRANKLIN.

TO OLIVER NEAVE.

On the Art of Swimming.

[No date.]

DEAR SIR,

I cannot be of opinion with you that it is too late in life for you to learn to swim. The river near the bottom of your garden affords a most convenient place for the purpose. And as your new employment requires your being often on the water, of which you have such a dread, I think you would do well to make the trial; nothing being so likely to remove those apprehensions as the consciousness of an ability to swim to the shore, in case of an accident, or of supporting yourself in the water till a boat could come to take you up.

I do not know how far corks or bladders may be useful in learning to swim, having never seen much trial of them. Possibly they may be of service in supporting the body while you are learning what is called the stroke, or that manner of drawing in and striking out the hands and feet that is necessary to produce progressive motion. But you will be no swimmer till you can place some confidence in the power of the water to support you; I would therefore advise the acquiring that confidence in the first place; especially as I have known several, who, by a little of the practice necessary for that purpose, have insensibly acquired the stroke, taught as it were by nature.

The practice I mean is this. Choosing a place where the water deepens gradually, walk coolly into it till it is up to your breast, then turn round, your face to the shore, and throw an egg into the water between you and the shore. It will sink to the bottom, and be easily seen there, as your water is clear. It must lie in water so deep as that you cannot reach it to take it up but by diving for it. To encourage yourself in order to do this, reflect that your progress will be from deeper to shallower water, and that at any time you may, by bringing your legs under you and standing on the bottom, raise your head far above the water. Then plunge under it with your eyes open, throwing yourself towards the egg, and endeavouring by the action of your hands and feet against the water to get forward till within reach of it. In this attempt you will find, that the water buoys you up against your inclination; that it is not so easy a thing to sink as you imagined; that you cannot but by active force get down to the egg. Thus you feel the power of the water to support you, and learn to confide in that power; while your endeavours to overcome it, and to reach the egg, teach you the

manner of acting on the water with your feet and hands, which action is afterwards used in swimming to support your head higher above water, or to go forward through it.

I would the more earnestly press you to the trial of this method, because, though I think I satisfied you that your body is lighter than water, and that you might float in it a long time with your mouth free for breathing, if you would put yourself in a proper posture, and would be still and forbear struggling; yet till you have obtained this experimental confidence in the water, I cannot depend on your having the necessary presence of mind to recollect that posture and the directions I gave you relating to it. The surprise may put all out of your mind. For though we value ourselves on being reasonable, knowing creatures, reason and knowledge seem on such occasions to be of little use to us; and the brutes, to whom we allow scarce a glimmering of either, appear to have the advantage of us.

I will, however, take this opportunity of repeating those particulars to you, which I mentioned in our last conversation, as, by perusing them at your leisure, you may possibly imprint them so in your memory as on occasion to be of some use to you.

1. That though the legs, arms, and head, of a human body, being solid parts, are specifically something heavier than fresh water, yet the trunk, particularly the upper part, from its hollowness, is so much lighter than water, as that the whole of the body taken together is too light to sink wholly under water, but some part will remain above, until the lungs become filled with water, which happens from drawing water into them instead of air, when a person in the fright attempts breathing while the mouth and nostrils are under water.

2. That the legs and arms are specifically lighter

than salt water, and will be supported by it, so that a human body would not sink in salt water, though the lungs were filled as above, but from the greater specific gravity of the head.

3. That therefore a person throwing himself on his back in salt water, and extending his arms, may easily lie so as to keep his mouth and nostrils free for breathing; and by a small motion of his hands may prevent turning, if he should perceive any tendency to it.

4. That in fresh water, if a man throws himself on his back, near the surface, he cannot long continue in that situation but by proper action of his hands on the water. If he uses no such action, the legs and lower part of the body will gradually sink till he comes into an upright position, in which he will continue suspended, the hollow of the breast keeping the head uppermost.

5. But if, in this erect position, the head is kept upright above the shoulders, as when we stand on the ground, the immersion will, by the weight of that part of the head that is out of water, reach above the mouth and nostrils, perhaps a little above the eyes, so that a man cannot long remain suspended in water with his head in that position.

6. The body continuing suspended as before, and upright, if the head be leaned quite back, so that the face looks upwards, all the back part of the head being then under water, and its weight consequently in a great measure supported by it, the face will remain above water quite free for breathing, will rise an inch higher every inspiration, and sink as much every expiration, but never so low as that the water may come over the mouth.

7. If therefore a person, unacquainted with swimming and falling accidentally into the water, could have pres-

ence of mind sufficient to avoid struggling and plunging, and to let the body take this natural position, he might continue long safe from drowning till perhaps help would come. For as to the clothes, their additional weight while immersed is very inconsiderable, the water supporting it, though when he comes out of the water, he would find them very heavy indeed.

But, as I said before, I would not advise you or any one to depend on having this presence of mind on such an occasion, but learn fairly to swim; as I wish all men were taught to do in their youth. They would, on many occurrences, be the safer for having that skill, and on many more the happier, as freer from painful apprehensions of danger, to say nothing of the enjoyment in so delightful and wholesome an exercise. Soldiers particularly should, methinks, all be taught to swim; it might be of frequent use either in surprising an enemy, or saving themselves. And if I had now boys to educate, I should prefer those schools (other things being equal) where an opportunity was afforded for acquiring so advantageous an art, which, once learned, is never forgotten.

I am, Sir, &c.

B. FRANKLIN.

TO M. DUBOURG.*

Modes of Swimming. — Answer to Inquiries. — Swimming with a Kite.

I am apprehensive, that I shall not be able to find leisure for making all the disquisitions and experiments

* Translated from M. Dubourg's edition of Franklin's Works, Vol. II p. 258. — EDITOR.

which would be desirable on this subject. I must, therefore, content myself with a few remarks.

The specific gravity of some human bodies, in comparison to that of water, has been examined by Mr. Robinson, in our Philosophical Transactions, Volume L., page 30, for the year 1757. He asserts, that fat persons with small bones float most easily upon the water.

The diving-bell is accurately described in our Transactions.

When I was a boy, I made two oval palettes, each about ten inches long, and six broad, with a hole for the thumb, in order to retain it fast in the palm of my hand. They much resembled a painter's palettes. In swimming I pushed the edges of these forward, and I struck the water with their flat surfaces as I drew them back. I remember I swam faster by means of these pallets, but they fatigued my wrists. I also fitted to the soles of my feet a kind of sandals; but I was not satisfied with them, because I observed that the stroke is partly given by the inside of the feet and the ancles, and not entirely with the soles of the feet.

We have here waistcoats for swimming, which are made of double sail-cloth, with small pieces of cork quilted in between them.

I know nothing of the *scaphandre* of M. de la Chapelle.

I know by experience, that it is a great comfort to a swimmer, who has a considerable distance to go, to turn himself sometimes on his back, and to vary in other respects the means of procuring a progressive motion.

When he is seized with the cramp in the leg, the method of driving it away is, to give to the parts affected a sudden, vigorous, and violent shock; which he may do in the air as he swims on his back.

During the great heats of summer there is no danger

in bathing, however warm we may be, in rivers which have been thoroughly warmed by the sun. But to throw one's self into cold spring water, when the body has been heated by exercise in the sun, is an imprudence which may prove fatal. I once knew an instance of four young men, who, having worked at harvest in the heat of the day, with a view of refreshing themselves plunged into a spring of cold water; two died upon the spot, a third the next morning, and the fourth recovered with great difficulty. A copious draught of cold water, in similar circumstances, is frequently attended with the same effect in North America.

The exercise of swimming is one of the most healthy and agreeable in the world. After having swam for an hour or two in the evening, one sleeps coolly the whole night, even during the most ardent heat of summer. Perhaps, the pores being cleansed, the insensible perspiration increases and occasions this coolness. It is certain that much swimming is the means of stopping a diarrhœa, and even of producing a constipation. With respect to those, who do not know how to swim, or who are affected with a diarrhœa at a season which does not permit them to use that exercise, a warm bath, by cleansing and purifying the skin, is found very salutary, and often effects a radical cure. I speak from my own experience, frequently repeated, and that of others, to whom I have recommended this.

You will not be displeased if I conclude these hasty remarks by informing you, that as the ordinary method of swimming is reduced to the act of rowing with the arms and legs, and is consequently a laborious and fatiguing operation when the space of water to be crossed is considerable; there is a method in which a swimmer may pass to great distances with much facility, by means of a sail. This discovery I fortunately made by accident, and in the following manner.

When I was a boy, I amused myself one day with flying a paper kite; and approaching the bank of a pond, which was near a mile broad, I tied the string to a stake, and the kite ascended to a very considerable height above the pond, while I was swimming. In a little time, being desirous of amusing myself with my kite, and enjoying at the same time the pleasure of swimming, I returned; and, loosing from the stake the string with the little stick which was fastened to it, went again into the water, where I found, that, lying on my back and holding the stick in my hands, I was drawn along the surface of the water in a very agreeable manner. Having then engaged another boy to carry my clothes round the pond, to a place which I pointed out to him on the other side, I began to cross the pond with my kite, which carried me quite over without the least fatigue, and with the greatest pleasure imaginable. I was only obliged occasionally to halt a little in my course, and resist its progress, when it appeared that, by following too quick, I lowered the kite too much; by doing which occasionally I made it rise again. I have never since that time practised this singular mode of swimming, though I think it not impossible to cross in this manner from Dover to Calais. The packet-boat, however, is still preferable.

B. FRANKLIN.

TO M. DUBOURG.*

On the free Use of Air.

London, 28 July, 1768.

I greatly approve the epithet which you give, in your letter of the 8th of June, to the new method of treating the small-pox, which you call the *tonic* or bracing method; I will take occasion from it to mention a practice to which I have accustomed myself. You know the cold bath has long been in vogue here as a tonic; but the shock of the cold water has always appeared to me, generally speaking, as too violent, and I have found it much more agreeable to my constitution to bathe in another element, I mean cold air. With this view I rise almost every morning, and sit in my chamber without any clothes whatever, half an hour or an hour, according to the season, either reading or writing. This practice is not in the least painful, but, on the contrary, agreeable; and, if I return to bed afterwards, before I dress myself, as sometimes happens, I make a supplement to my night's rest of one or two hours of the most pleasing sleep that can be imagined. I find no ill consequences whatever resulting from it, and that at least it does not injure my health, if it does not in fact contribute much to its preservation. I shall therefore call it for the future a *bracing* or *tonic* bath.

B. FRANKLIN.

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* Translated from M. Dubourg's edition of Franklin's Works, Vol. II. p. 310.—EDITOR.

A SCHEME

FOR A

NEW ALPHABET AND REFORMED MODE OF SPELLING;

WITH

REMARKS AND EXAMPLES.

For the nature and intention of this alphabet, I must refer to what Dr. Franklin has himself said upon the subject, in answer to Miss Stevenson's objections; as the reader may understand the whole in an hour or two. It is necessary to add, that the new letters used in the course of printing this paper, are exactly copied from the *manuscript* in my possession; there being no provision for a distinction in the character as *written* or *printed*. I have no other way, therefore, of marking the scored parts of the manuscript (answering to *italics*,) than by placing such passages between inverted commas. As to *capitals*, I should have provided for them by means of larger types, but the form of some of them would have made them too large for the page. However, were the author's general system ever adopted, nothing would be easier than to remedy this particular. — B. V.

The new characters, used in this edition, have been cut and executed after the model of those contained in Mr. Vaughan's edition, where the scheme was first published. — EDITOR.

TABLE OF THE

Characters.	Sounded respectively, as in the words in the column below.
o	Old.
a	John, folly; awl, ball.
a	Man, can.
e	Men, lend, name, lane.
i	Did, sin, deed, seen.
u	Tool, fool, rule.
y	um, un; as in umbrage, unto, &c., and as in *er*.
h	Hunter, happy, high.
g	Give, gather.
k	Keep, kick.
ſh	(sh) Ship, wish.
ŋ	(ng) ing, repeating, among.
n	End.
r	Art.
t	Teeth.
d	Deed.
l	Ell, tell.
s	Essence.
z	(ez) Wages.
ϑ	(th) Think.
ϑ	(dh) Thy.
f	Effect.
v	Ever.
b	Bees.
p	Peep.
m	Ember.

REFORMED ALPHABET.

Names of Letters as expressed in the reformed Sounds and Characters.	Manner of pronouncing the Sounds.
o	The first VOWEL naturally, and deepest sound; requires only to open the mouth, and breathe through it.
a	The next requiring the mouth opened a little more, or hollower.
a	The next, a little more.
e	The next requires the *tongue* to be a little more elevated.
i	The next still more.
u	The next requires the *lips* to be gathered up, leaving a small opening.
y	The next a very short vowel, the sound of which we should express in our present letters thus, *uh*; a short, and not very strong *aspiration*.
huh	A stronger or more forcible aspiration.
gi	The first CONSONANT; being formed by the *root of the tongue*; this is the present hard *g*.
ki	A kindred sound; a little more acute; to be used instead of the hard *c*.
ish	A new letter wanted in our language; our *sh*, separately taken, not being proper elements of the sound.
ing	A new letter wanted for the same reason:—These are formed *back in the mouth*.
en	Formed *more forward* in the mouth; the *tip of the tongue* to the *roof* of the mouth.
r	The same; the tip of the tongue a little loose or separate from the roof of the mouth, and vibrating.
ti	The tip of the tongue more forward; touching, and then leaving, the roof.
di	The same; touching a little fuller.
el	The same; touching just about the *gums* of the *upper teeth*.
es	This sound is formed by the breath passing *between* the moist end of the *tongue* and the *upper teeth*.
ez	The same, a little denser and duller.
eh	The tongue under, and a little *behind*, the upper teeth; touching them, but so as to let the breath pass between.
eh	The same; a little fuller.
ef	Formed by the *lower lip* against the upper teeth.
ev	The same; fuller and duller.
b	The *lips full together*, and *opened* as the air passes out.
pi	The same; but a thinner sound.
em	The *closing* of the lips, while the *e* [here annexed] is sounding.

REMARKS ON THE ALPHABETICAL TABLE.

o to *huh*	It is endeavoured to give the alphabet a *more natural order;* beginning first with the simple sounds formed by the breath, with none or very little help of tongue, teeth, and lips, and produced chiefly in the windpipe.
g k	Then coming forward to those, formed by the roof of the tongue next to the windpipe.
r n *t d*	Then to those, formed more forward, by the fore part of the tongue against the roof of the mouth.
l *s z*	Then those, formed still more forward, in the mouth, by the tip of the tongue applied first to the roots of the upper teeth.
ɦ *ɦ*	Then to those, formed by the tip of the tongue applied to the ends or edges of the upper teeth.
f *v*	Then to those, formed still more forward, by the under lip applied to the upper teeth.
b *p*	Then to those, formed yet more forward, by the upper and under lip opening to let out the sounding breath.
m	And lastly, ending with the shutting up of the mouth, or closing the lips, while any vowel is sounding.

In this alphabet *c* is *omitted* as unnecessary; *k* supplying its hard sound, and *s* the soft; *k* also supplies well the place of *q*, and, with an *s* added, the place of *x*; *q* and *x* are therefore omitted. The vowel *u*, being sounded as *oo*, makes the *w* unnecessary. The *y*, where

used simply, is supplied by *i*, and, where as a diphthong, by two vowels; that letter is therefore omitted as useless. The jod *j* is also omitted, its sound being supplied by the new letter *ſi*, *ish*, which serves other purposes, assisting in the formation of other sounds; thus the *ſi* with a *d* before it gives the sound of the jod *j* and soft *g*, as in "James, January, giant, gentle," "*dſieems, dſianueri, dſiqiant, dſientel*"; with a *t* before it, it gives the sound of *ch*, as in "cherry, chip," "*tſieri, tſiip*"; and, with a *z* before it, the French sound of the jod *j*, as in "jamais," "*zſiame.*"

Thus the *g* has no longer *two different* sounds, which occasioned confusion, but is, as every letter ought to be, confined to one. The same is to be observed in *all* the letters, vowels, and consonants, that wherever they are met with, or in whatever company, their sound is always the same. It is also intended, that there be *no superfluous* letters used in spelling; that is, no letter that is not sounded; and this alphabet, by six new letters, provides, that there be no distinct sounds in the language *without letters* to express them. As to the difference between *short and long vowels*, it is naturally expressed by a single vowel where short, a double one where long; as for "mend," write "mend," but for "remain'd," write "remeen'd;" for "did," write "did," but for "deed," write "diid," &c.

What in our common alphabet is supposed the third vowel, *i*, as we sound it, is as a *diphthong*, consisting of two of our vowels joined; viz. *ɥ* as sounded in "unto," and *i* in its true sound. Any one will be sensible of this, who sounds those two vowels *ɥ i* quick after each other; the sound begins *ɥ* and ends *ii*. The true sound of the *i* is that we now give to *e* in the words "deed, keep —."*

* The copy, from which this is printed, ends in the same abrupt way

EXAMPLES.

So huen sym endfiel, byi divyin kamand,
Uiħ ryizin tempests fieeks e gilti land,
(Sytfi az av leet or peel Britania past,)
Kalm and siriin hi dryivs ħi fiuriys blast;
And, pliiz'd ħ' almyitis ardyrs tu pyrfarm,
Ryids in ħi huyrluind and dyirekts ħi starm.

So ħi piur limpid striim, huen faul uiħ steens
av ryfiin tarents and disendin reens,
Uyrks itself kliir; and az it ryns rifyins;
Til byi digriis, ħi flotin miryr fiyins,
Riflekts iitfi flaur ħat an its bardyr groz,
And e nu hev'n in its feer byzym fioz.

FROM MISS MARY STEVENSON TO B. FRANKLIN.

Kensiŋtyn, 26 *Septembyr*, 1768.

Diir Syr,

yi hav transkryib'd iur alfabet, &c., huitfi yi ħink myit bi av syrvis tu ħoz, hu uifi to akuyir an akiuret pronynsiefiyn, if ħat kuld bi fiks'd; byt yi si meni inkanviiniensis, az uel az difikyltis, ħat uuld atend ħi briŋiŋ iur letyrs and arħagrafi intu kamyn ius. aal aur etimalodfiiz uuld be last, kansikuentli ui kuld nat asyrteen ħi miiniŋ av meni uyrds; ħi distinkfiyn, tu, bituiin uyrds av difyrent miiniŋ and similar saund uuld bi iusles, ynles ui liviŋ ryiters pyblifi nu

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with the above, followed by a considerable blank space; so that more perhaps was intended to be added by our author.—B. V.

iidifiyns. In fiart yi biliiv ui myst let piipil spel an in ȝeer old ue, and (az ui fyind it iisiiest) du ȝi seem aurselves. With ease and with sincerity I can, in the old way, subscribe myself,

Dear Sir,

Your faithful and affectionate servant,

M. S.

TO MISS MARY STEVENSON.

Diir Madam,

ȝi abdfiekfiyn iu meek to rektifyiiŋ aur alfabet, "ȝat it uil bi atended uiȝ inkanviniensiz and difikyltiz," iz e natural uyn; far it aluaz akyrz huen eni refarmefiyn is propozed; hueȝyr in rilidfiyn, gyvernment, laz, and iven daun az lo az rods and huil karidfiiz. ȝi tru kuestfiyn ȝen, is nat hueȝyr ȝaer uil bi no difikyltiz ar inkanviniensiz, byt hueȝyr ȝi difikyltiz mê nat bi syrmaunted; and hueȝyr ȝi kanviniensiz uil nat, an ȝi huol, bi grêtyr ȝan ȝi inkanviniensiz. In ȝis kes, ȝi difikyltiz er onli in ȝi biginiŋ av ȝi praktis; huen ȝê er uyns ovyrkym, ȝi advantedfiez er lastiŋ. — To yiȝyr iu ar mi, hu spel uel in ȝi prezent mod, yi imadfin ȝi difikylti av tfiendfiiŋ ȝat mod far ȝi nu, iz nat so grêt, byt ȝat ui myit pyrfektli git ovyr it in a uiiks ryitiŋ. — Az to ȝoz hu du nat spel uel, if ȝi tu difikyltiz er kympêrd, viz. ȝat av titfiiŋ ȝem tru speliŋ in ȝi prezent mod, and ȝat av titfiiŋ ȝem ȝi nu alfabet and ȝi nu speliŋ akardiŋ to it, yi am kanfident ȝat ȝi latyr uuld bi byi far ȝi liist. ȝê natyrali fal into ȝi nu meȝyd alreadi, az mytfi az ȝi imperfekfiyn av ȝêr alfabet uil admit av; ȝêr prezent bad speliŋ iz onli bad,

bikaz kantreri to ȝi prezent bad ruls; yndyr ȝi nat ruls it uuld bi gud. — ȝi difikylti av lyrniŋ to spel uel in ȝi old uê iz so grêt, ȝat fiu atên it; ȝauzands and ȝauzands ryitiŋ an to old edfi, uiȝaut ever biiŋ ebil to akuyir it. 'T iz, bisyidz, e difikylti kantinuali inkriisiŋ, az ȝi saund graduali veriz mor and mor fram ȝi speliŋ; and to farenyrs it mêks ȝi lyrniŋ to pronauns aur laguedfi, az riten in aur buks, almost impasibil.*

Nau az to "ȝi inkanviniensiz" iu menfiyn. — ȝi fyrst iz, ȝat "aal aur etimalodfiiz uuld bi last, kansikuentli ui kuld nat asyrteen ȝi miiniŋ av meni uyrds." — Etimalodfiiz er at prezent veri ynsyrteen; byt sytfi az ȝê er, ȝi old buks uuld stil prizyrv ȝem, and etimolodfiists uuld ȝêr fyind ȝem. Uyrds in ȝi kors av tyim, tfiendfi ȝer miinigs, az uel az ȝer speliŋ and pronynsiefiyn; and ui du nat luk to etimalodfii far ȝer prezent miinigs. If yi fiuld kal e man e neev and e vilen, hi uuld hardli bi satisfyid uiȝ myi teliŋ him, ȝat uyn av ȝi uyrds oridfiinali signifyid onli e lad ar syrvant; and ȝi yȝyr, an yndyr plauman, ar ȝi inhabitant av e viledfi. It iz fram prezent iusedfi onli, ȝi miiniŋ av uyrds iz to bi dityrmined.

Iur sekynd inkanviniens iz, ȝat "ȝi distinkfiyn bituiin uyrds av difyrent miiniŋ and similar saund uuld bi distrayid." — ȝat distinkfiyn iz alreadi distrayid in pronaunsiŋ ȝem; and ui rilyi an ȝi sens alon av ȝi sentens to asyrteen, huitfi av ȝi several uyrds, similar in saund, ui intend. If ȝis iz syfifient

* Dr. Franklin used to lay some little stress on this circumstance, when he occasionally spoke on the subject. "A dictionary, formed on this model, would have been serviceable to him, he said, even as an American;" because, from the want of public examples of pronunciation in his own country, it was often difficult to learn the proper sound of certain words, which occurred very frequently in our English writings, and which of course every American very well understood as to their meaning. — B. V.

in ȝi rapiditi av diskors, it uil bi mutfi mor so in riten sentenses, huitfi mê bi red lezfiurli, and atended to mor partikularli in kes av difikylti, ȝan ui kan atend to e past sentens, huyil e spikyr iz hyryiiŋ ys alaŋ uiȝ nu uyns.

Iur ȝyrd inkanviniens iz, ȝat "aal ȝi buks alredi riten uuld bi iusles." — ȝis inkanviniens uuld onli kym an graduali, in e kors av edfies. Iu and yi, and yȝyr nau liviŋ ridyrs, uuld hardli farget ȝi ius av ȝem. Piipil uuld loŋ lyrn to riid ȝi old ryitiŋ, ȝo ȝê praktist ȝi nu. — And ȝi inkanviniens iz nat greter, ȝan huat hes aktuali hapend in a similar kes, in Iteli. Farmerli its inhabitants aal spok and rot Latin; az ȝi laŋuedfi tfiendfid, ȝi speliŋ falo'd it. It iz tru ȝat, at prezent, e miir ynlern'd Italien kanat riid ȝi Latin buks; ȝo ȝê er stil red and yndyrstud byi meni. Byt, if ȝi speliŋ had nevyr bin tfiendfied, hi uuld nau hev faund it mytfi mor difikylt to riid and ryit hiz on laŋuadfi; far riten uyrds uuld hev had no rilêfiyn to saunds, ȝê uuld onli kev stud far ȝiŋs; so ȝat if hi uuld ekspres in ryitiŋ ȝi yidia hi hez, huen hi saunds ȝi uyrd Vescovo, *hi myst iuz ȝi letterz* Episcopus. — *In fiart, huatever ȝi difikyltiz and inkanviniensiz nau er, ȝê uil bi mor iizili syrmaunted nau, ȝan hiraftyr; and sym tyim ar yȝyr, it myst bi dyn; ar aur ryitiŋ uil bikym ȝi sêm uiȝ ȝi Tfiyiniiz,* az to ȝi difikylti av lyrniŋ and iuziŋ it. And it uuld alredi hev bin sytfi, if ui had kantinud ȝi Saksyn speliŋ and ryitiŋ, iuzed byi our forfaȝers.*

yi am, myi diir frind, iurs afekfiynetli,

B. FRANKLIN.

Lyndyn,
Kreven-striit, Sept. 28, 1768.

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* Chinese.

PETITION OF THE LETTER Z.*

From "The Tatler," No. 1778.

To the Worshipful Isaac Bickerstaff, Esquire, Censor-General.

The petition of the letter Z, commonly called *Ezzard*, *Zed*, or *Izard*, most humbly showeth;

That your petitioner is of as high extraction, and has as good an estate, as any other letter of the Alphabet;

That there is therefore no reason why he should be treated as he is, with disrespect and indignity;

That he is not only actually placed at the tail of the Alphabet, when he had as much right as any other to be at the head; but is by the injustice of his enemies totally excluded from the word WISE; and his place injuriously filled by a little hissing, crooked, serpentine, venomous letter, called S, when it must be evident to your worship, and to all the world, that W, I, S, E, do not spell *Wize*, but *Wise*.

Your petitioner therefore prays, that the Alphabet may by your censorial authority be reversed; and that in consideration of his long-suffering and patience he may be placed at the head of it; that *s* may be turned out of the word *Wise*; and the petitioner employed instead of him.

And your petitioner, as in duty bound, shall ever pray, &c. &c.

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* The date of this *jeu d'esprit* in imitation of "The Tatler," from which it purports to be an extract, is not known. It is inserted in this place as being somewhat kindred in its subject to the preceding article. — EDITOR

Mr. Bickerstaff, having examined the allegations of the above petition, judges and determines, that Z be admonished to be content with his station, forbear reflections upon his brother letters, and remember his own small usefulness, and the little occasion there is for him in the Republic of Letters, since S whom he so despises can so well serve instead of him.

TO JOHN WINTHROP.

Telescope and Books for Harvard College.

London, 11 March, 1769.

DEAR SIR,

At length after much delay and difficulty I have been able to obtain your telescope, that was made by Mr. Short before his death. His brother who succeeds in the business has fitted it up and completed it. He has followed the business many years at Edinburgh, is reckoned very able, and therefore I hope every thing will be found right; but, as it is only just finished, I have no time left to get any philosophical or astronomical friends to examine it, as I intended, the ship being on the point of sailing, and a future opportunity uncertain. Enclosed is his direction-paper for opening and fixing it.

I have not yet got the bill of the price. It is to be made from the deceased Mr. Short's book of memorandums and orders, in which he entered this order of ours, and, as it is supposed, the price. I do not remember, it is so long since, whether it was one hundred pounds, or one hundred guineas; and the book is in the hands of the executor as I understand. When I have the account, I shall pay it as I did Bird's for

the transit instrument, which was forty guineas, and then shall apply for the whole to Mr. Mauduit. By the way, I wonder that I have not heard from you of the receipt of that instrument, which went from hence in September by Captain Watt. I hope it got safe to hand and gave satisfaction. The ship was the same that Mr. Rogers went in, who I hear is arrived; and by him too I sent the Philosophical Transactions, with a number of copies of your paper as printed separately. But I have no letter from you since that by the young gentleman you recommended to me, grandson to Sir William Pepperell, which I think was dated about the beginning of October, when you could not have received them.

By a late ship, I sent your College a copy of the new edition of my Philosophical Papers; and others, I think, for yourself and for Mr. Bowdoin. I should apologize to you for inserting therein some part of our correspondence without first obtaining your permission; but, as Mr. Bowdoin had favored me with his consent for what related to him, I ventured to rely upon your good-nature, as to what related to you, and I hope you will forgive me.

I have got from Mr. Ellicot the glasses, &c. of the long Galilean telescope, which he presents to your college. I put them into the hands of Mr. Nairne, the optician, to examine and put them in order. I thought to have sent them by this ship, but am disappointed. They shall go by the next, if possible.

There is nothing new here in the philosophical way at present.

With great and sincere esteem, I am, Dear Sir,

Your most obedient and humble servant,

B. FRANKLIN.

P. S. There is no prospect of getting the duty acts

repealed this session, if ever. Your steady resolutions to consume no more British goods may possibly, if persisted in, have a good effect another year. I apprehend the Parliamentary resolves and address will tend to widen the breach. Enclosed I send you Governor Pownall's speech against those resolves; his name is not to be mentioned. He appears to me a hearty friend to America, though I find he is suspected by some on account of his connexions.

TO M. DUBOURG.*

Introducing Dr. Lettsom.

London, 30 August, 1769.

This letter will be forwarded to you by Dr. Lettsom, a young American physician of much merit, and one of the peaceable sect of Quakers; you will therefore at least regard him as a curiosity, even though you should have embraced all the opinions of the majority of your countrymen concerning these people.

B. Franklin.

ON VENTILATION.

Written by Mr. Small, the Surgeon, but containing Dr. Franklin's Observations on the Subject.

I do not know that we have in any author particular and separate directions concerning the ventilating of hospitals, crowded rooms, or dwellinghouses; or the

* Translated from M. Dubourg's edition of Franklin's Works, Vol. II. p. 314.—Editor.

making of proper drains for carrying off stagnant or putrid water. The want of such general information on these subjects, has induced me to endeavour to recollect all I can of the many instructive conversations I have had upon these matters with that judicious and most accurate observer of nature, Dr. Benjamin Franklin. I do this in hopes that either the Doctor himself, or some other person well qualified for the task, may follow the example set in so masterly a manner by Sir John Pringle, Baronet, when speaking on the preservation of the health of seamen.

It has long been observed, that if a number of persons are shut up in a small room, of which the internal air has little or no communication with the external, the respiration of those who are so confined renders by degrees the air of that room effete, and unfit for the support of life.

Dr. Franklin was, if I mistake not, the first who observed, that respiration communicated to the air a quality resembling the mephitic; such as the *Grotto del Cane* near Naples. The air impressed with this quality rises only to a certain height, beyond which it gradually loses it. The amendment begins in the upper part, and descends gradually until the whole becomes capable of sustaining life. The Doctor confirmed this by the following experiment. He breathed gently through a tube into a deep glass mug, so as to impregnate all the air in the mug with this quality. He then put a lighted *bougie* into the mug; and upon touching the air therein the flame was instantly extinguished; by frequently repeating this operation, the *bougie* gradually preserved its light longer in the mug, so as in a short time to retain it to the bottom of it; the air having totally lost the bad quality it had contracted from the breath blown into it.

At the same time that the lower part of the air is thus affected, an acrid, noxious quality may be communicated to its upper part in the room, occasioned by the volatile putrescent effluvia of the persons enclosed therein. "It is surprising," says Sir John Pringle, in his "Observations on the Diseases of the Army," fourth edition, p. 109, "in how few days the air will be corrupted in close and crowded wards; and what makes it hard to remedy the evil, is the difficulty of convincing either nurses or the sick themselves, of the necessity of opening the windows and doors at any time for a supply of fresh air."

It may be inferred from the above account of mephitic air, that such air can be but little altered by a ventilator in the ceiling of a room; and Dr. Franklin justly concluded, that in crowded rooms, and especially in bedrooms in dwellinghouses, a current of air should be kept up in the lower part of the rooms, to carry off what is thus affected. He approved of the use of chimneys for this purpose, especially when the current is quickened by a fire. Even when there is not any fire in the chimney, a current of air is constantly kept up in i by its ascending or descending in the flue, as the weight of the internal or external air preponderates. This creates a kind of tide in the flue, conducing much to the healthiness of air in rooms; and hence we may see the injudiciousness of having chimney-boards which fit closely, and thereby prevent a salutary circulation in the air. Hence also in warm weather we may account for liquors or other things kept in a chimney being cooled, and more so if means are used to create an evaporation around them.

Every person has an atmosphere of his own, heated by the warmth of his body, which can be dissipated only by motion in the circumambient air. Thus in

warm weather, wind cools the body, by carrying off the personal atmosphere, and promoting at the same time a more free evaporation of the effluvia arising from the body. This creates a great degree of coolness on the skin. The personal atmosphere can be but little affected by a ventilator in the ceiling of a room, unless the admission of external air is so directed as to act principally on the air surrounding those in the room. Dr. Franklin, when consulted on ventilating the House of Commons, represented that the personal atmosphere surrounding the members might be carried off by making outlets in the perpendicular parts of the seats, through which the air might be drawn off by ventilators, so placed, as to accomplish this without admitting any by the same channels. It will appear from what has been said, that windows placed high in the walls of churches or rooms intended for large assemblies, can contribute but little towards correcting the mephitic quality of the lower part of the air, or towards carrying off the personal atmospheres.

The experiments made for ventilating crowded rooms by that most beneficent of men, the Reverend Dr. Stephen Hales, bring evident proof how much the upper part of the air in such places is vitiated by the volatile putrescent effluvia arising from the persons present in such rooms. He at the same time showed an easy and effectual way to carry off such vitiated air. His ventilators were, however, attended with the inconveniency of occasioning smoky chimneys, by drawing off so much air, that there was not a sufficiency left to keep a current strong enough to carry the smoke up the chimney, unless a door or window was left open. The circulating ventilators in windows were intended for refreshing the air in rooms, without affecting the current of air up the chimney; but they did not affect

the mephitic air, nor the higher air near the ceiling of lofty rooms, which is most vitiated with putrescent particles; and they were besides often out of repair.

Instead of either of these, Dr. Franklin proposed that openings should be made close to the ceilings of rooms communicating with a flue, which should ascend in the wall close to the flues of the chimneys, and, where it can be done conveniently, close to the flue of the kitchen chimney; because the fire, burning pretty constantly there, would keep the sides of that flue warmer than those of the other chimneys; whereby a quicker current of air would be kept up in the ventilating flue. Such a flue might be carried from the vaults or under ground offices. This would render them drier, without altering their temperature much as to heat or cold. These ventilating flues would cause a constant discharge of the volatile putrescent *effluvia* without interfering with the current of air up the chimneys; while the current towards the chimney would carry off the *mephitic* air below. These ventilating flues would be peculiarly beneficial in bedrooms of which the ceilings are low.

Dr. Franklin mentioned an instance of a number of Germans, who on their arrival in Pennsylvania were obliged to live in a large barn; there being at that time no other place of residence fit for them. Several small windows were made on both sides of the barn under the eaves. These windows were kept constantly open, even during a severe frost in the winter; and this without any detriment to the health of the Germans. Prejudice, said he, has raised so great a dread against cold air in England, that such openings would make every person shudder at the thought of being exposed to so great a degree of cold; and therefore I did not dare to recommend a practice, the good effects of which I had known. The dormitory for the youths

of Westminster School is a similar instance; for the glass put in their high lofty windows is soon broken, but seldom repaired; yet without prejudice to the health of the youths.

There is a channel by which much of the vitiated air escapes, and is but little attended to. Whoever looks at the ceilings of rooms in old houses, will soon discover the traces of the rafters, by a difference in color, in parts of the ceiling,* — and deposits in it part of its contents, which discolors the intervals between the joists. In the British Museum there is a remarkable instance of the inconveniency of the want of this outlet. The ceiling of one of the rooms in that house is covered with a picture, or painted cloth. The room continues warm with little fire; but the air soon affects the respiration of valetudinarians, as was often remarked by that accurate observer, Dr. G. Knight, late principal librarian.

An attentive observer will soon be convinced, that there is a current of warm air which ascends in the room from the chimney, while a fire burns. Dr. Franklin showed that this was the case, by the following experiment. He suspended, by a thread, a piece of pasteboard cut in a spiral form. The thread was fastened to the chimney-piece, so that the pasteboard, drawn out in a spiral form, came near to the edge of the chimney. The constant current of warm air, heated by the fire, gave a continued circular motion to the pasteboard. This warm air ascending to the ceiling, there spread, and kept a constant motion in the upper part of the air. The warm air thus ascending, coming into contact with the cool walls, and being thereby condensed, becomes heavier, and so falls along the sides of the walls. Also the glass in windows, being

* Some words seem to be here wanting to complete the sense. The piece is re-printed from the edition of W. T. Franklin. — Editor.

exposed to the temperature of the external air, in cold weather becomes colder than any other part of the room; therefore more sensibly descends, as may be seen by approaching a lighted *bougie* to a window. The flame is then carried downwards by the air; or, if the flame is extinguished, the smoke will more clearly show this truth, by descending along the window till it meets the air of an equal temperature. This will be the case, however tight the window; and the more so, the brighter and stronger the fire is, and the colder the external air; the circulation of the air being thereby quickened. This accounts for the familiar caution of avoiding to sit in or near a window. This circulation of the air is yet more evidently proved by the following instance. When there is a bright strong fire in a close room, open the door and present immediately a lighted candle to the upper part of the door-way, the flame will bend outward; though warm air in the higher part rushes out, lower the candle gradually, and the strength of the current outward will lessen by degrees, as the candle is lowered, till it comes to a space in which the flame shall rise upright; continue to lower the candle gradually, and then the current of cold air inward will gradually increase and more strongly bend the flame of the candle inward. This will be the case even in frosty and windy weather. May it not be inferred from this circumstance of so strong a current of air outwards in the upper part of the door-way, that an opening over or in the upper part of the door in the ward of a hospital might be of advantage, especially if there is no ventilating flue in the ceiling? By such means a circulation of the air in the upper part of the ward could be constantly kept up; and thereby a vent would be given to the volatile putrescent parti-

cles. This vent might remain open at all times, without any prejudice to the patients.

What is said on this subject by Dr. John Armstrong, a gentleman no less remarkable for his benevolence than for his judgment and fine taste, may be properly mentioned here. "A constant circulation of fresh air is so necessary, so important in fevers and in all feverish disorders, that it ought to be particularly considered in the construction of houses. It would be well, if in all the apartments of every house, but especially in bed-chambers, the upper sashes of every window were contrived to let down; for by this means the admission of fresh air would at all times be perfectly safe; except during a raw, damp, foggy night; as the body, even when under such a sweat as could not without danger be interrupted, may receive all the refreshing, restorative, and invigorating influence of the air, without being exposed to a stream of it; meantime, where this is wanting, the best method to supply it is by drawing the bed-curtains close, now and then, for a few minutes at a time, while a free passage is made to the foul air, by opening the doors and windows."—*Medical Essays*, p. 22.

The noxious vapors that fill a sick room are not only offensive, but dangerous to those who continue in it for any time. If dangerous to people then in health, how detrimental must they be to one oppressed and struggling under an enfeebling disease! It is a common thing in a campaign to distribute the sick soldiers, ill of malignant fevers, in open barns, where the putrid volatile poison is in a short time dissipated.

There is, in a volume of the *Mémoires et Observations recueillies par la Société Economique de Berne*, a letter concerning the health of the inhabitants of the *Pays de Vaud;* part of which I beg to present here,

as bearing a near analogy to this subject. The letter is written by a most accurate and judicious clergyman. "One fact," says he, "deserves to be noticed. Taking one year with another, a greater proportional number always die in towns than in villages. But whence comes it, that, when epidemic diseases prevail, the mortality takes quite a different road; that is, it is much more considerable in villages than in towns? I have taken great pains to find out the cause of this phenomenon, and am apt to impute the difference to the difference of habitations. The poor in cities and great towns dwell in houses originally not intended for them; but which, being so old and past repairing, as to be no longer tenantable by persons at their ease, fall to the lot of the lower class of people. In these houses the rooms are spacious, cold as ice, where the air plays freely around, with doors and windows that do not half shut. The inhabitants of these shattered houses are pitied; and yet the very circumstance of their being out of repair, is what contributes to the health of those who live in them, and facilitates their cure when diseases reign."

The more I see of hospitals, the more I am convinced of the great want of instructions on duly ventilating them. It is surprising to see what little attention has been paid in some hospitals about London to this article, which have been built since the importance of ventilation has been well known. In all of them there is too great a distance between the windows and the ceilings, where the volatile putrescent particles may remain till they become very acrid. With pleasure I here do justice to the judgment and precaution of Messrs. Adam, in the manner of ventilating the great room built by them for the meeting of the Society for the Encouragement of Arts, &c., by leaving spaces

between the panes of glass in the sky-lights, the panes overlaying each other. These spaces being concealed from the eye, do not alarm those fearful of cold air, and keep the room constantly sweet.

The hospitals the most judiciously built in this respect, which I have seen, are those in Philadelphia and in Lyons. In the hospital in Philadelphia the wards are two stories high, with two rows of windows in each, the upper row being kept generally open; and the windows in the hospital at Lyons are very lofty, so that the upper sashes may be for the most part kept open. Both hospitals are by this means perfectly sweet; so sweet, that a military gentleman, who went with me into the hospital at Lyons, and was unaccustomed to sick rooms, declared, that the air in the ward was not disagreeable to him, though it contained a considerable number of sick. Indeed they were kept very clean. I am sorry to say this is not the case in any one of our hospitals.

The naval hospital at Gibraltar is a square, which in a hot climate is itself a great imperfection, as the air within the square must in summer especially be greatly heated; and, as if they had studied to keep the cool air out of the wards, the windows open into the square only; whereas, if the west side had been left open, the wards might have received the cool breezes from the Bay. The sick are lodged in long galleries not sufficiently divided to have the patients in separate wards, and no openings to carry off the putrid air lodged among the rafters which support the roof.

On my arrival in the Island of Minorca, as surgeon to the royal artillery there, I was surprised at the neglect of my predecessors in that office, in regard to ventilating the hospital. There were no openings in the wards in which the sick lay; but the windows and

doors, which were necessarily shut every night, to prevent the irregularities soldiers might be guilty of. Where chimneys had been, they were built up to prevent the expense of fires; and thus, during the night, the sick lay in an absolutely confined air. The consequence was, that, when the nurse opened the wards in the morning, she was obliged to withdraw instantly; for the highly-infected air often brought on a vomiting. In this case I applied to our most worthy and ingenious chief engineer, the late Colonel Mackellar, for leave to use such means as might create a circulation of air in all the wards. In this he readily concurred, and ordered the necessary alterations.

In each ward in which the flue of the chimney remained, an opening of about four or five inches square was made through the wall into the flue, as near the ceiling as possible. Round holes of about three inches diameter were cut low in each door, covered with a sliding flap to shut the holes occasionally. In some of the wards there never had been chimneys. In these, holes were cut through the wall close to the ceiling, which opened into a common passage; and, when two such wards were contiguous, a hole was cut through the dividing wall as well as in the door of each ward. One of the wards, in which there had not been a chimney, and which was arched with stones, was constantly so damp that no use was made of it. The walls and arch were covered with green moss. They were afterwards scraped, to clear them of the moss which retained moisture, and then covered with lime. This room became so dry, that though locked up for three months, during which I was confined with the gout, books and papers which had remained in it, were at the end of that time perfectly dry. The generally agreeable effects of this opening can scarcely be conceived; the

wards, and indeed the whole hospital, being rendered perfectly sweet, greatly to the benefit of the sick, as well as to the pleasure of the attendants.

The barracks in the square of the Castle St. Philip, in which are lodged the detachment of the regiment of artillery doing duty there, are dry, except that, being built of stone, they collect moisture on every sudden change of the air from cold to warm. Each barrack opens into the square, and is divided into three apartments. The part next the door has the whole height, and in it their arms and necessaries are kept. The inner part, being about one half, is divided into two floors. In the lower room they cook, each barrack having a mess or family in it, some of whom sleep in it. The fire, and the free access between it and the door, keep up a due circulation here. In the upper room most of the men sleep under a stone arch, the room being little more than six feet high in the centre, and therefore much lower in the sides. Under that arch from four to six or eight persons sleep, especially when there are children. This room is very stifling, there being little circulation of air in it, more especially in calm, warm weather, such as the nights generally are there in summer. In order to create a circulation of air in this upper room, openings were made into the flues of the chimneys of the lower room, as near the centre of the arch as possible, the chimneys being in the corner of the rooms below. In general these openings drew very well, and gave great relief, especially to those who had weak or diseased lungs. The proper remedy here would have been, to have had small flues made near the flues of the chimneys below, could it have been done. This measure is too much neglected in all barracks.

Whoever may on any future occasion have the di-

rection of military hospitals, is already furnished with such judicious directions by my learned friends Sir John Pringle, Baronet, and Dr. Donald Munro, that, were I to say any thing on that subject, I could only copy whole pages from them. Sir John Pringle's speech on giving the gold medal of the Royal Society to Captain Cook, in which he took occasion to point out the means of preserving the health of seamen, is equally deserving of commendation.

The healthiness of buildings does not perhaps depend more on the due ventilation of the rooms, than it does on the dryness of the situation, and of the foundation. Sir John Pringle, in the first part of his *Observations on the Diseases of the Army*, has given several instances of this truth. But as every man who regards his own life and health, or the lives and health of others, should be well acquainted with that work, I shall refer to the original. I have often lamented that the first part of that book, describing the natural consequences of the situation of places and their effects on health, has not been published separately; because it might thereby become of more general use to every man who leads a country life, or resorts thither frequently to enjoy quiet; for, being part of a book professedly treating of diseases, few think of consulting it, except those whose business it is to cure diseases.

However inviting the situation may be, and whatever may be the quality of the ground on which houses are built, generally drains should be made all round the house deeper than the foundation of the building, to carry off the superfluous moisture; even the moisture that may be lodged under ground; for it is essentially necessary that the lower part of the house be kept continually dry.

The advantages of drains or sewers are remarkably

felt in London, which, before the fire of London, was frequently afflicted with contagious malignant fevers. Before that period all the waste water and filth remained above ground; and the people, as Erasmus complained, were very inattentive to keeping their houses clean. The wooden houses projected so much over the then very narrow streets, that the air became almost stagnant, and must have been loaded with putrid effluvia, there being very little circulation or current in the air, thus confined, to carry off these effluvia.

Before the city was rebuilt, that ingenious architect Sir Christopher Wren, planned and built the common sewers, as they continue to this day; and they are a lasting monument of his judgment and attention to the health and welfare of its inhabitants. These, together with the removal of signs and sign-posts, new paving and cleansing the streets, have been attended with such happy effects, that London and Westminster are now ranked among the most healthy spots in the island, for grown persons whose lungs can bear the cloud of smoke which generally hovers over them; and thus the apparent great calamity of a fire became a singular blessing to the city of London.

The quantity of water brought into the city by the New River and other water-works, which runs daily to waste, helps to cleanse and keep the common sewers sweet, and thereby contributes much to the healthiness of the city. Though foreign to the subject, it may be observed, that till the Restoration there were few gardens about London for supplying kitchen herbs. These became more numerous after that period, and still more so after the Revolution; a number of Dutch gardeners coming to England at that time. The quantity of vegetables supplied by these gardens contribute greatly to the healthiness of the citizens.

Rome would not perhaps have become mistress of so extraordinary an empire, situated as that city is near marshy grounds, had not the common sewers, which still attract the admiration of all travellers, been so early and judiciously built by Tarquinius Priscus, who may for that reason be called a second founder of Rome. The ancient Romans were particularly attentive to the draining and cultivating of these marshes, and they soon became the granary of ancient Rome; but, being neglected during the invasions of the barbarous nations, they are now the reproach and just chastisement of the supine indolence and inactivity of the modern Romans.

Gravel, which is generally reckoned a dry and healthy foundation to build upon, is found by experience not to deserve that character at all times, unless deep drains are made to carry off the water of heavy rains long continued; for by such rains the gravel may be so charged with water, especially in flat grounds, that the lower parts of the houses erected on such soils may prove damp. In all the flat grounds along the Thames the cellars are often filled with water after heavy rains; and, if the water continues there stagnant, till the animal or vegetable substances mixed with it begin to putrefy, aches, agues, and putrid fevers are the natural consequences. Though Kensington Palace stands high, and on a declivity, yet, when King George the Second continued there till late in October, the lower parts of the house became damp, occasioned by the want of drains; and the servants became aguish. Stones which absorb and retain water, as the *Cantoon* stone in *Minorca* do, are in this respect similar to gravel. There was a remarkable instance of this in a magazine cut out of a solid rock of *Cantoon* stone in Georgetown, in Minorca. The magazine was covered with a well-limed arch and roof. Yet, when the winter rains began to fall in No-

vember, the magazine was filled with water, as high as it was cut out of the rock. When drains were made to carry off the water, the magazine then became and continued to be sufficiently dry.

Might not low grounds on the banks of rivers, similar to those in Flanders, and so justly and judiciously complained of by Sir John Pringle, be rendered more healthy, by drains dug as deep as low-water mark in the adjacent rivers? Sluices might be made in the banks of the river, to prevent the tides or floods from entering into the drains. It would be advisable to cover the drains, to prevent the noxious vapors arising from putrid animal or vegetable substances, which generally rot in open ditches. The earth thrown out of the drains might serve to cover them, when the channels for carrying off the water are properly constructed. By these means no surface would be lost for the growth of vegetables.

Willows, alders, and such trees as delight in a moist or wet soil, may be planted on the banks of ditches, if any such are permitted to remain open, that their leaves may correct the putrid vapors arising from the stagnant water in the ditches. I fear, however, that in the autumn, when the effects of putrid vapors are most severely felt, the leaves of these trees, being then hardened by age, may in a great measure lose the power of correcting the putrid vapors. The late summer shoots may afford aid till the equinoxial rains clear the ditches of all filth. That trees have not the power of proving an effectual remedy against these putrid exhalations, the frequency of agues in the low countries, in every such season, is a sufficient proof. If such trees grow on the banks of ditches, they should be kept in a pollard state, to admit of a free circulation of air.

An observation of Dr. Franklin's deserves a place

here, especially as it is not generally attended to. The opinion is indeed against it. The banks of rivers which have a quick motion, and run on a clear sandy bottom, are very agreeable and healthy situations; but the sides of rivers which have oozy bottoms, or marshy banks, or which are in the neighbourhood of extensive marshes, are to be avoided. When necessity or any peculiar advantage obliges people to build near such bad neighbours, the south side, says the Doctor, is the most eligible; because the warm southerly winds, which promote a tendency to putrefaction, and are the most frequent, blow the noxious vapors from the buildings; whereas the northerly winds, which blow but seldom compared with the former, and which generally blow strongly, check putrefaction, and speedily carry off noxious vapors.

It is now well known that the stench arising from stationary privies, may be prevented by a cheap and easy method. The excrements may be received in tubs, so closely connected with the seat, that no air can pass. The lower ends of the tub should be sunk below the surface of water contained in proper cisterns. The excrements are soon dissolved in water, and so carried off, *every time the privy is washed, which should be as often as it is used.*

In towns the stench of the common sewers is sometimes very offensive. This may be prevented by interrupting the current of air through them by means of stink-traps; the construction and utility of which, are of late years well known in London. As sand or other filth may be apt to lodge in the deepened place, it should be so contrived, as to be easily come at, in order to clear away every obstruction.

Let me add here to the method of correcting bad water, proposed by Dr. Munro, in his *Essay on the*

Means of preserving the Health of Soldiers, the following easy method of keeping water clear and sweet, ascertained by several experiments, made some years ago by the Society for the Encouragement of Arts, &c., in London. The method is to mix clay with the water in such quantities, that when the clay is dissolved, the hand immersed under the surface of the water shall not be seen. The clay subsiding, carries down with it all the impurities, and, in a manner burying them, prevents their communicating any bad taste or smell to the water, which thereby continues long clear and sweet. Clay may probably correct stagnant water, and thereby preserve it clear and good in dry seasons, and may thus become very useful, where there is no running water. If any bad taste or smell remains after the use of the clay, it may be carried off by one of the ventilators recommended for that purpose by the Reverend Dr. Hales. The clear water may be drawn off by a siphon or a cock, placed high enough not to touch the clay.

A. S.

TO MISS MARY STEVENSON.

Method of contracting Chimneys.—Modesty in Disputation.

Craven Street, Saturday Evening, past ten.

The question you ask me is a very sensible one, and I shall be glad if I can give you a satisfactory answer. There are two ways of contracting a chimney; one, by contracting the opening *before* the fire; the other, by contracting the funnel *above* the fire. If the

funnel above the fire is left open in its full dimensions, and the opening before the fire is contracted; then the coals, I imagine, will burn faster, because more air is directed through the fire, and in a stronger stream; that air which before passed over it, and on each side of it, now passing *through* it. This is seen in narrow stove chimneys, when a sacheverell or blower is used, which still more contracts the narrow opening. But if the funnel only *above* the fire is contracted, then, as a less stream of air is passing up the chimney, less must pass through the fire, and consequently it should seem that the consuming of the coals would rather be checked than augmented by such contraction. And this will also be the case, when both the opening *before* the fire, and the funnel *above* the fire are contracted, provided the funnel above the fire is more contracted in proportion than the opening before the fire.

So you see I think you had the best of the argument; and, as you notwithstanding gave it up in complaisance to the company, I think you had also the best of the dispute. There are few, though convinced, that know how to give up, even an error, they have been once engaged in maintaining; there is therefore the more merit in dropping a contest where one thinks one's self right; it is at least respectful to those we converse with. And indeed all our knowledge is so imperfect, and we are from a thousand causes so perpetually subject to mistake and error, that positiveness can scarce ever become even the most knowing; and modesty in advancing any opinion, however plain and true we may suppose it, is always decent, and generally more likely to procure assent. Pope's rule,

"To speak, though sure, with seeming diffidence,"

is therefore a good one; and, if I had ever seen in your

conversation the least deviation from it, I should earnestly recommend it to your observation.

I am, &c.

B. FRANKLIN.

FROM JOHN EWING TO B. FRANKLIN.*

Transmitting Observations of the Transits of Venus and Mercury. — Proposal for an Observatory in Philadelphia.

Philadelphia, 4 January, 1770.

SIR,

Our Philosophical Society have at length ordered me to draw out an account of our observations of the transits of Venus and Mercury, to be transmitted to you as our President, through whose hands we think they may be most conveniently communicated to the learned societies of Europe, to whom you may suppose they will be agreeable. The reason of their not coming sooner to hand was a rash agreement of the Society not to send them abroad, until we had printed them in our own Transactions. But finding that there was some reason to suspect, that partial accounts of them had been transmitted to England by some of our members privately, which possibly might be inaccurate and not much to be depended on, the Society have thought proper to reconsider that hasty agreement, and to send them without farther loss of time. We hope, however, that they will be as soon with you, as those that have been made in South America and the East Indies. I

* Dr. Ewing was a clergyman of Philadelphia, and for many years Provost of the University of Pennsylvania. He was a scholar of large attainments in various branches of science and literature, as well as in theology. — EDITOR.

have enclosed to you two copies, one directed to the Astronomer Royal, Mr. Maskelyne, at Greenwich, and the other to be transmitted by you to any of the learned societies in Europe you shall think proper.

You are well acquainted with the serenity of our air, and the advantages we enjoy above many countries in Europe for making celestial observations, could a fixed observatory be established in Philadelphia. I have mentioned this matter to Mr. Maskelyne, and referred him to you for advice how to bring it into execution, should it meet with your approbation. You are well acquainted with the views of our Assembly, and the unreasonableness of expecting that they would lay out any of the public money for such a purpose, unless it was recommended by you to them; as they place an unreserved confidence in your judgment concerning what measures would tend to the reputation of the province, the advancement of useful knowledge, and the benefit of the public in general. Geography, navigation, and the arts that depend upon them, are daily reaping advantages from the astronomical observations made in the different observatories of Europe. Should you think it of any consequence, that we in this infant country might bear any part in these things, your known character and ability to judge in these matters must necessarily have so much influence with our Assembly, as to induce them to bear at least a part in the expense of it. And possibly Mr. Maskelyne might suggest some means of affording assistance from home, from the consideration of its being made subservient to his observatory at Greenwich, and put under his general direction.

I have not mentioned this proposal to any person here, but Mr. Coombe, and that under a promise that he will not speak of it, as I apprehend there is a pro-

priety in its coming from you and the Astronomer Royal. So that if it does not meet with your approbation, pray let it sleep, and be so kind as to excuse the trouble given you by, Sir, your most obedient servant,

JOHN EWING.

P. S. Mr. Coombe tells me, that he has mentioned the above proposal to his son, with a desire that he should speak to you on the subject.

Please send me authentic accounts of the observations of the transit of Venus made in as many places as you can conveniently procure, if your leisure from more important business will permit, that we may here also endeavour to solve the curious problem of the sun's parallax. If it would not be trespassing too much, I should also be much obliged to you for an account of Messrs. Mason's and Dixon's determinations on the length of a degree of latitude here, as they have been employed by the Royal Society to measure it in the Lower Counties.*

TO NEVIL MASKELYNE, ASTRONOMER ROYAL.

Containing an Account of Professor Winthrop's Observation of the Transit of Mercury over the Sun, November 9th, 1769.

READ AT THE ROYAL SOCIETY, JANUARY 10th, 1771.

Craven Street, 12 February, 1770.

DEAR SIR,

I have just received a letter from Mr. Winthrop, dated December 7th, containing the following account, viz.

* A brief and interesting account of the observations, in America, of the transits of Venus and Mercury, including those mentioned above, may be found in Professor Renwick's LIFE OF RITTENHOUSE, contained in the seventh volume of the *Library of American Biography*. — EDITOR.

"On Thursday, the 9th of November, I had an opportunity of observing a transit of Mercury. I had carefully adjusted my clock to the apparent time, by correspondent altitudes of the Sun, taken with the quadrant for several days before, and with the same reflecting telescope as I used for the transit of Venus.* I first perceived the little planet making an impression on the sun's limb at 2^h 52′ 41″; and he appeared wholly within at 53′ 58″ apparent time. The sun set before the planet reached the middle of his course; and for a considerable time before sunset, it was so cloudy, that the planet could not be discerned. So that I made no observations of consequence, except that of the beginning, at which time the sun was perfectly clear. This transit completes three periods of forty-six years, since the first observation of Gassendi at Paris, in 1631."

I am, Sir, with great esteem,
Your most obedient servant,
B. FRANKLIN.

TO MICHAEL HILLEGAS.

Respecting covering Houses with Copper.

London, 17 March, 1770.

DEAR SIR,

I received your favor of November 25th, and have made inquiries, as you desired, concerning the copper covering of houses. It has been used here in a few instances only, and the practice does not seem to gain ground. The copper is about the thickness of a common playing-card; and, though a dearer metal than

* See Philosophical Transactions, Vol. LIX. p. 352.

lead, I am told, that, as less weight serves, on account of its being so much thinner, and as slighter woodwork in the roof is sufficient to support it, the roof is not dearer on the whole, than one covered with lead.

It is said, that hail and rain make a disagreeable drumming noise on copper; but this I suppose is rather fancy; for the plates being fastened on the rafters must, in a great measure, deaden such sound. The first cost, whatever it is, will be all, as a copper covering must last for ages; and, when the house decays, the plates will still have intrinsic worth. In Russia, I am informed, many houses are covered with plates of iron tinned, such as our tin pots and other vases are made of, laid on over the edges of one another like tiles; and which, it is said, last very long, the tin preserving the iron from much decay by rusting. In France and the Low Countries, I have seen many spouts or pipes for conveying the water down from the roofs of houses, made of the same kind of tin plates soldered together; and they seem to stand very well.

With sincere regard, I am
Yours, &c.

B. FRANKLIN.

FROM JOHN EWING TO B. FRANKLIN.

Philadelphia, 14 June, 1770.

SIR,

I received your very agreeable letter, in which you acknowledge the receipt of our observations of the transit of Venus. I herewith send you a few copies of them, as they are printed in our Transactions, and I suppose in a more perfect form than that in which they were sent before, as that was done in a hurry, and I

have not a copy of what I sent. If what you received before is not printed in the Philosophical Transactions, please to request Mr. Maskelyne to insert them as they are now sent, unless he should think it better to abridge or alter them, as to their dress or form, which he has full liberty to do, as he is much better acquainted with publications of this kind than I can be supposed to be.

I mentioned to our Society your proposal to purchase the Transactions of the learned societies in Europe, and they have taken the matter under their consideration. They approved of your reasoning on the subject, when I read it to them; and nothing will prevent their coming into the resolution, if their poverty does not.

I hope, that before this time Mr. Maskelyne has given you an estimate of the expense and apparatus necessary for erecting an observatory here, where we are blessed with so happy a serenity of air for astronomical observations. I have not yet mentioned this matter to our Society, but wait until I hear farther from you; and would still choose, that, when proposed, it should come from you and the Astronomer Royal, to whose judgment our Society pay the greatest respect in these matters.

Please to accept of my hearty thanks for the perusal of your last volume of the Philosophical Transactions. I shall deliver it safely to Mrs. Franklin.

When the observations of the transit of Venus come to hand from the East Indies, the North of Europe, or from South America, I shall be much obliged to you for a copy of them, as I am anxious to know how they correspond with ours.

I am, Sir, your most obedient and very humble servant,

JOHN EWING.

P. S. Please to deliver one of the copies with my compliments to the Astronomer Royal, the Reverend Mr. Maskelyne.

TO SAMUEL RHOADS.

On securing Houses from Fire.

London, 26 June, 1770.

DEAR FRIEND,

It is a long time since I had the pleasure of hearing from you directly. Mrs. Franklin has indeed now and then acquainted me of your welfare, which I am always glad to hear of. It is, I fear, partly, if not altogether my fault, that our correspondence has not been regularly continued. One thing I am sure of, that it has been from no want of regard on either side, but rather from too much business, and avocations of various kinds, and my having little of importance to communicate.

One of our good citizens, Mr. Hillegas, anxious for the future safety of our town, wrote to me some time since, desiring I would inquire concerning the covering of our houses here with copper. I sent him the best information I could then obtain, but have since received the enclosed from an ingenious friend, who is what they call here a civil engineer. I should be glad you would peruse it, think of the matter a little and give me your sentiments of it. When you have done with the paper, please to give it to Mr. Hillegas. I am told by Lord Despencer, who has covered a long piazza, or gallery, with copper, that the expense is charged in this account too high; for his cost but one shilling and ten pence per foot, all charges included. I suppose his copper

must have been thinner. And, indeed, it is so strong a metal, that I think it may well be used very thin.

It appears to me of great importance, to build our dwellinghouses, if we can, in a manner more secure from danger by fire. We scarcely ever hear of fire in Paris. When I was there I took particular notice of the construction of their houses, and I did not see how one of them could well be burnt. The roofs are slate or tile, the walls are stone, the walls generally lined with stucco or plaster, instead of wainscot, the floors of stucco, or of six square tiles painted brown, or of flag stone, or of marble; if any floors were of wood, it was of oak wood, which is not so inflammable as pine. Carpets prevent the coldness of stone or brick floors offending the feet in winter, and the noise of treading on such floors overhead, is less inconvenient than on boards.

The stairs too, at Paris, are either stone or brick, with only a wooden edge or corner for the step; so that on the whole, though the Parisians commonly burn wood in their chimneys, a more dangerous kind of fuel than that used here, yet their houses escape extremely well, as there is little in a room that can be consumed by fire except the furniture; whereas in London, perhaps scarcely a year passes in which half a million of property and many lives are not lost by this destructive element. Of late, indeed, they begin here to leave off wainscoting their rooms, and instead of it cover the walls with stucco, often formed into pannels like wainscot, which, being painted, is very strong and warm. Stone staircases too, with iron rails, grow more and more into fashion here; but stone steps cannot, in some circumstances, be fixed; and there, methinks, oak is safer than pine; and I assure you, that in many genteel houses here, both old and new, the stairs and floors are oak, and look extremely well. Perhaps solid oak

for the steps would be still safer than boards; and two steps might be cut diagonally out of one piece.

Excuse my talking to you on a subject with which you must be so much better acquainted than I am. It is partly to make out a letter, and partly in hope, that, by turning your attention to the point, some methods of greater security in our future building may be thought of and promoted by you, whose judgment I know has deservedly great weight with our fellow-citizens. For, though our town has not hitherto suffered very greatly by fire, yet I am apprehensive, that some time or other, by a concurrence of unlucky circumstances, such as dry weather, hard frost, and high winds, a fire then happening may suddenly spread far and wide over our cedar roofs, and do us an immense mischief. I am, yours, &c.

B. FRANKLIN.

Paper referred to in the preceding Letter.

THE carpentry of the roof, being formed with its proper descents, is, in the first place, sheeted or covered with deals, nailed horizontally upon the rafters, after the same manner as when intended to be covered with lead. The sheets of the copper for this covering are two feet by four, and for covering the slopes of the roof are cast so thin, as to weigh eight or nine pounds, and for covering the flats or gutters, ten or eleven pounds each, or about one pound, or a pound and a quarter, to the superficial foot.

A string of strong cartridge paper (overlapping a little at its joints) is regularly tacked down upon the sheeting, under the copper covering, as the work proceeds from eaves to ridge. It prevents the jingling sound of hail or rain falling upon the roof, and answers another purpose to be mentioned by and by.

In order to show the regular process of laying down the roof, we must begin with fastening two sheets together lengthwise. The edges of two sheets are laid down so as to lap or cover each other an inch, and a slip of the same copper, about three inches and a half broad, called the reeve, is introduced between them. Four oblong holes, or slits, are then cut or punched through the whole, and they are fastened or riveted together by copper nails, with small round shanks and flat heads. Indents are then cut one inch and three quarters deep upon the seam at top and bottom. The right-hand sheet and the reeve are then folded back to the left. The reeve is then folded to the right, and the sheets being laid on the roof in their place, it is nailed down to the sheeting with flat-headed, short copper nails. The right hand sheet is then folded over the reeve to the right, and the whole beat down flat upon the cartridge paper, covering the sheeting, and thus they are fastened and laid in their places, by nailing down the reeve only; and by reason of the oblong holes through them and the reeve, have a little liberty to expand or contract with the heat and cold, without raising themselves up from the sheeting, or tearing themselves or the fastening to pieces.

Two other sheets are then fixed together, according to the first and second operations above, and their seam, with the reeve, introduced under the upper ends of the seam of the former, so as to cover down about two inches upon the upper ends of the former sheets; and so far the cartridge paper is allowed to cover the two first sheets. This edge of the paper is dipped in oil, or in turpentine, so far before its application, and thus a body between the sheets is formed impenetrable to wet, and the reeve belonging to the two last sheets is nailed down to the sheeting as before, and the left-

hand sheet is turned down to the right. Four sheets are now laid down, with the seam or joint rising to the ridge; and thus the work is continued, both vertically and horizontally, till the roof be covered, the sides and ends of each sheet being alternately, each way, undermost and uppermost.

The price for copper, nails, and workmanship, runs at about eight pounds ten shillings per hundred weight, or two shillings and three pence per foot, superficial, exclusive of the lappings; and about two shillings and eight pence per foot upon the whole; which is rather above half as much more as the price of doing it well with lead.

TO THOMAS PERCIVAL.*

On the different Quantities of Rain which fell at different Heights over the same Ground.

On my return to London I found your favor of the 16th of May (1771). I wish I could, as you desire, give you a better explanation of the phenomenon in question, since you seem not quite satisfied with your own; but I think we want more and a greater variety of experiments in different circumstances, to enable us to form a thoroughly satisfactory hypothesis. Not that I make the least doubt of the facts already related, as I know both Lord Charles Cavendish and Dr. Heberden

* This letter is without date, but was probably written in the year 1771, since it was in answer to a letter dated in May of that year. It was first printed in the *Memoirs of the Literary and Philosophical Society of Manchester*, Vol. II. p. 110, having been communicated to the Society by Dr. Percival, and read on the 21st of January, 1784. — EDITOR.

to be very accurate experimenters; but I wish to know the event of the trials proposed in your six queries; and also, whether in the same place where the lower vessel receives nearly twice the quantity of water that is received by the upper, a third vessel placed at half the height will receive a quantity proportionable. I will however endeavour to explain to you what occurred to me, when I first heard of the fact.

I suppose it will be generally allowed, on a little consideration of the subject, that scarce any drop of water was, when it began to fall from the clouds, of a magnitude equal to that it has acquired, when it arrives at the earth; the same of the several pieces of hail; because they are often so large and so weighty, that we cannot conceive a possibility of their being suspended in the air, and remaining at rest there, for any time, how small soever; nor do we conceive any means of forming them so large, before they set out to fall. It seems then, that each beginning drop, and particle of hail, receives continual addition in its progress downwards. This may be several ways; by the union of numbers in their course, so that what was at first only descending mist, becomes a shower; or by each particle, in its descent through air that contains a great quantity of dissolved water, striking against, attaching to itself, and carrying down with it such particles of that dissolved water, as happen to be in its way; or attracting to itself such as do not lie directly in its course by its different state with regard either to common or electric fire; or by all these causes united.

In the first case, by the uniting of numbers, larger drops might be made, but the quantity falling in the same place would be the same at all heights; unless, as you mention, the whole should be contracted in falling, the lines described by all the drops converging,

so that what set out to fall from a cloud of many thousand acres, should reach the earth in perhaps a third of that extent, of which I somewhat doubt. In the other cases we have two experiments.

1. A dry glass bottle filled with very cold water, in a warm day, will presently collect from the seemingly dry air that surrounds it a quantity of water, that shall cover its surface and run down its sides; which perhaps is done by the power wherewith the cold water attracts the fluid common fire that had been united with the dissolved water in the air, and drawing the fire through the glass into itself, leaves the water on the outside.

2. An electrified body, left in a room for some time, will be more covered with dust than other bodies in the same room not electrified, which dust seems to be attracted from the circumambient air.

Now we know that the rain, even in our hottest days, comes from a very cold region. Its falling sometimes in the form of ice shows this clearly; and perhaps even the rain is snow or ice, when it first moves downwards, though thawed in falling; and we know that the drops of rain are often electrified. But those causes of addition to each drop of water, or piece of hail, one would think could not long continue to produce the same effect; since the air, through which the drops fall, must soon be stripped of its previously dissolved water, so as to be no longer capable of augmenting them. Indeed very heavy showers, of either, are never of long continuance; but moderate rains often continue so long as to puzzle this hypothesis; so that upon the whole I think, as I intimated before, that we are yet hardly ripe for making one.

B. FRANKLIN.

FROM W. SMALL TO B. FRANKLIN.

Opinions of the Ancient Physicians respecting Catarrhs, or Colds.

Birmingham, 10 August, 1771.

DEAR SIR,

The reason of your having no sooner received the quotation from Celsus, is, that I wished to employ my very first leisure in looking into several other ancient books for passages to the same purpose, and to send you all together. But Mr. Keir having told me of your desire to see that immediately, you have it almost alone.

In the article "*De Tabe*" in his third book, treating of the cure, he says, "Cavendæ *distillationes*, ne, si quid cura levarit, exasperent; et *ob id, vitanda cruditas*, simulque et sol et frigus."* Here indigestion seems to be reckoned the principal cause. If you have not attended to that particular before, you may be surprised to find sunshine among the causes of colds, but such is the doctrine of all the ancients. A passage about the instruments of cure in coughs may perhaps amuse you, "Utilis etiam in omni tussi est peregrinatio, navigatio longa, loca maritima, *natationes*."†

From several things in Xenophon and in Plato, the prevailing opinion in their time seems to have been, that what we now commonly call colds and catarrhs, arose almost solely from excess and indolence. On this account Xenophon says, that in Persia in the days of Cyrus, to spit or to blow a nose was infamous.

* "Great caution should be observed, when relief is once obtained, lest catarrhs are made worse; indigestion also, as well as exposure to the sun and cold air, ought to be avoided."

† "In all coughs it is found beneficial to take long journeys and voyages, to reside on the sea-coast, and to use sea-baths."

Plato often commends simple spare diet, but in one place he says it prevents all *catarrhs*. Whether he means precisely what we call catarrhs, however, in that passage, may be doubted.

I do not recollect any absolutely express testimony in your favor from Hippocrates. Mucus (of the nose) and saliva he judges to be signs of repletion; and he maintains that persons who drink and eat sparingly are free from diseases occasioned by moisture. Abundance may be found in Galen to your purpose. A modern author, who ought to have understood this subject, for he has written so great a book about catarrhs,* that you had better have twenty colds than read it, is of your opinion. "Illa, illa, inquam, cibi potusque abundantia citat catarrhos. Eosdem abigunt frugalitas et labor. Ut ex luxu et otio nascuntur catarrhi, ita horum medicina est in sobrietate, in continentiâ, in exercitationibus corporis, in mentis tranquillitate. Quotusquisque vero hæc precepta, has leges vivendi custodit? Homo frugi est rara avis, &c. *Hinc* nemo mortalium fere est sine catarrhis." †

Mr. Boulton will soon present you with one of the boxes with invisible hinges. He has astonished our rural philosophers exceedingly by calming the waves *à la Franklin*. ‡

I am trying some experiments in relation to the

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* This book upon the Catarrh is probably that of *Schneiderus*, consisting of four volumes, 4to.

† "Eating and drinking too much is sufficient of itself, I say, to produce catarrhs. Temperance and active pursuits on the contrary drive them away. As catarrhs are produced by luxury and indolence, so the remedy for these is to observe sobriety, continence, exercise of body and tranquillity of mind. How few observe these precepts! Temperance indeed is very rare. Hence very few escape catarrhs."

‡ That is, by means of oil, according to the experiments mentioned in this volume under the date of January 27th, 1773, p. 355.—EDITOR.

improvement of telescopes; should they answer you shall hear of them.

I beg you will make my most respectful compliments to the fellow travellers who were with you here, and believe me to be,

With the highest regard, dear Sir,

Your much obliged and most obedient servant,

W. SMALL.

TO M. LE ROY.

Intended French Voyage to the North Pole. — Mr. West, President of the Royal Society. — Messrs. Banks and Solander.

London, 20 April, 1772.

DEAR SIR,

I received your favor of March 5th, by M. Dazeux, and shall be glad of any opportunity of doing him service. It gave me great pleasure to learn by him, that you are well and happily married, on which I give you joy. It is after all the most natural state of man.

Mr. West, our President, concerning whom you make inquiry, is esteemed a good antiquarian, but has not distinguished himself in any other branch of science. He is a member of Parliament, was formerly Secretary to the Treasury, and is very rich.*

I am glad to hear that a voyage is intended from France to the North Pole. The world owes much to the noble spirit with which your nation pursues the

* James West was President of the Royal Society from November, 1768, till his death in July, 1772. He possessed a very extensive library of rare and valuable books, which were sold by auction after his death. His curious collection of manuscripts was sold to the Marquis of Lansdown, of whom they were purchased by Parliament, and they now make a part of the *Lansdown Manuscripts* in the British Museum. — EDITOR.

improvement of knowledge, and to the liberality with which you communicate what you acquire to the rest of mankind. I hope your philosophers on this voyage will be able to discover more clearly the cause of the Aurora Borealis, and a passage round the North of America.

I suppose care has been taken to make their ships very strong, that they may bear thumping among the ice. My best wishes will attend them for their success and safe return.

Messrs. Banks and Solander are to sail with two ships in about a fortnight for the South. They expect to be out near four years. They present their compliments, and are pleased with the notice you honor them with in your letter to me. Sir John Pringle continues well, and presents his respectful compliments to you. I am, with the most perfect esteem,

Dear Sir, yours, &c.

B. FRANKLIN.

TO JOSEPH PRIESTLEY.

Electrical Experiments. — Imperfection of the Abbé Nollet's Electrical Machine. — One on an improved Construction sent to M. Le Roy.

London, 4 May, 1772.

DEAR SIR,

I think with you that there cannot be the least occasion for my explaining your method of impregnating water with fixed air to Messrs. Banks and Solander, as they were present, and I suppose are as well acquainted with it as myself; however, I shall readily do it, if they think it necessary. I am glad you intend to improve and publish the process.

You must go half an inch farther with your spark to exceed what I showed here with my Philadelphia machine in 1758, to Lord Charles Cavendish and others, who judged them to be nine inches. My cushion was of buckskin, with a long damp flap, and had a wire from it through the window down to the iron rails in the yard; the conductor of tin four feet long and about four inches diameter. So powerful a machine had then never been seen in England before, as they were pleased to tell me. A machine was made from mine for Mr. Timmer, and was afterwards in the possession of Lord Morton. A more convenient construction I have never since seen, except that of yours. I intend soon to repeat Barletti's experiments, being provided with the requisites, and shall let you know the result.

I should be glad to see the French translation of your book. Can you conveniently lend it to me when you have perused it? I fancy it was translated at the request of Abbé Nollet by a friend and disciple of his; as I know there was one (whose name I have forgotten), that used to translate for him extracts of English electrical books.

The Abbé's machine was a very bad one, requiring three persons to make the smallest experiment, one to turn the great wheel, and one to hold hands on the globe. And the effect after all was but weak. De Lor had a similar one, and invited me to see him exhibit to the Duchess of Rochefoucauld; but, the weather being a little warm, he could perform nothing, scarce obtaining a spark.

This inconvenience must have occasioned his making fewer experiments, and of course his not being so easily convinced. M. Le Roy however got early possession of the truth, and combated for it with Nollet; yet I think the Academy rather favored the latter.

Le Roy will, I suppose, now confute this translator, for I have just seen a letter of his to Mr. Magelhaens, thanking him for sending so excellent an electrical machine to France, (it is one of the plate ones,) which he has improved so as to produce the positive and negative electricities separately or together at the same time. "De façon," says he, "qu'on peut faire toutes les expériences possibles sur l'une ou l'autre de ces deux électricités. Enfin on étoit si eloigné de connoître les phénomènes de ces deux électricités ici, faute de machines commodes de les demontrer, que beaucoup des gens ont été étonnés de voir avec quelle évidence ils établissent la distinction de ces deux électricités," &c. This letter is of the 5th instant.

My best wishes attend you and yours. I am ever, with great respect, my dear friend,

Yours most sincerely,

B. FRANKLIN.

FROM JOSEPH PRIESTLEY TO B. FRANKLIN.

Experiments on Air.

Leeds, 3 June, 1772.

DEAR SIR,

You make me very happy by the near prospect of seeing you and Sir John Pringle at Leeds. I shall be entirely at liberty to receive you, and I hope you will contrive to stay as long as possible in this town and neighbourhood. I thank you for the *Native of New England.** I had casually seen the same paper, and

* Probably alluding to a piece entitled *Toleration in Old England and New England*, and signed A NEW ENGLAND MAN. This piece was first printed in "The London Packet," June 3d, 1772. See Vol. II. p. 112. — EDITOR.

was particularly struck with it, without having any suspicion of *Poor Richard* being the author of it. I am obliged to you for your advice with respect to the *Dedication*, and shall comply with it; but some other alterations, besides what you noted, must be made in it, if it be addressed to Lord Sandwich only.

I am intent upon the prosecution of my experiments on *air*, and since I wrote to you have observed several remarkable appearances. That very extraordinary kind of air, which Dr. Hales got from Walton pyrites, and which I had despaired of procuring, I get from all the metals I have yet tried, by means of spirit of nitre. It is quite transparent; but a mixture of it and common air is red for a considerable time, in which the whole quantity is greatly reduced in bulk. A mixture of this and fixed air is not turbid. This air alone is reduced above one half by a mixture of iron filings and brimstone standing in it, whereas common air is diminished only about one fifth in the same process.

When I have the pleasure of seeing you, I shall acquaint you with some other remarkable properties of this new kind of air. In the mean time you will do me a very important service by procuring for me, and bringing along with you, a little of highly concentrated *marine acid*. There is none to be got here; and, using a weaker sort in the solution of gold, I was obliged to apply a considerable degree of heat, the consequence of which was, that, the acid menstruum suddenly boiling, my hands, face, clothes, and the walls of the room have been great sufferers by it, as, I am afraid, I shall be able to show you. A pennyweight of gold, which I had bought for the purpose, was also lost.

As a reward for this damage, I preserved about three ounce measures of *air* extracted from gold, which I believe was never seen before, and have the prodigious

satisfaction of finding, that it has the very same properties with that which is produced from copper. If I had studied *Poor Richard* in time, I should not have indulged myself in these expenses; but bad habits are not easily corrected. If, however, the passion be not kept up by considerable success, frugality and an attention to a growing family will, at length, get the better of experimenting, and then I shall write nothing but *Politics* or *Divinity*, to furnish the Bishop of Llandaff with more quotations for his future invectives against the Dissenters.

The French translation of my "History of Electricity" I borrowed of Mr. Walsh; but, as it will be of some use to me in a future edition of my work, I think to purchase it. In the mean time Mr. Walsh will have no objection to your having it for what time you please, and I can give it to you when you are here.

I am surprised that the French electricians should not have been able to provide themselves with better machines. I am confident that plates will never answer so well as globes or cylinders. I am, with my respectful compliments to Sir John Pringle,

Dear Sir, yours sincerely,

JOSEPH PRIESTLEY.

P. S. I wish you could bring Dr. Price with you.

FROM JOSEPH PRIESTLEY TO B. FRANKLIN.

Curious Experiments on Air, and Discoveries of various Properties.

Leeds, 1 July, 1772.

DEAR SIR,

I presume that by this time you are arrived in London, and I am willing to take the first opportunity of

informing you, that I have never been so busy, or so successful in making experiments, as since I had the pleasure of seeing you at Leeds.

I have fully satisfied myself, that air, rendered in the highest degree noxious by breathing, is restored by sprigs of mint growing in it. You will probably remember the flourishing state in which you saw one of my plants. I put a mouse in the air, in which it was growing, on the Saturday after you went away, which was seven days after it was put in, and it continued in it five minutes without showing any sign of uneasiness, and was taken out quite strong and vigorous, when a mouse died after being not two seconds in a part of the same original quantity of air, which had stood in the same exposure without a plant in it. The same mouse, also, that lived so well in the restored air, was barely recoverable after being not more than one second in the other. I have also had another instance of a mouse living fourteen minutes without being at all hurt in little more than two ounce measures of another quantity of noxious air, in which a plant had grown.

I have completely ascertained the restoration of air, in which tallow or wax candles, spirit of wine, or brimstone matches, have burned out by the same means.

The *nitrous air*, which I showed you, I found to be an admirable test of air that is fit for breathing. It makes this air red and turbid, but no other that I have tried. I took air, in which a mouse had putrefied, which was in the highest degree noxious and fetid, and also a quantity of fixed air. The nitrous air, admitted to each of these kinds of air separately, made no sensible alteration in them; but, when they were mixed (which I discovered to make a wholesome air), the nitrous air made the mixture turbid and diminished the bulk of it, as in common air, though not in the same

degree. A mouse put into this mixture lived five minutes without uneasiness, when, if it had been put into either of them separately a few minutes before, it would have died in a few seconds.

Air, that has passed through hot charcoal, has many, perhaps all the properties of air that has been diminished by other processes. It extinguishes flame, kills animals, and is not diminished or made turbid by a mixture of nitrous air.

But the observation, that pleases me more than any I ever made, is the diminution of air by the crystallization (I believe) of quicksilver and the nitrous acid. This effect both precedes and follows the generation of nitrous air from the same mixture. This I suspect to be the case with other crystallizations.

I have observed many other things, which I have not room to mention at present.

I am, with great respect, dear Sir,

Yours sincerely,

JOSEPH PRIESTLEY.

FROM JOHN WALSH TO B. FRANKLIN.*

Experiments for ascertaining the Electrical Properties of the Torpedo.

La Rochelle, 12 July, 1772.

DEAR SIR,

It is with particular satisfaction I make to you my first communication, that the effect of the Torpedo ap-

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* Mr. Walsh was the first person, who ascertained from a series of experiments, that the shock communicated by the species of ray-fish called the *Torpedo*, is the same as that derived from the Leyden jar when charged with electricity. He had received directions from Dr.

pears to be absolutely electrical, by forming its circuit through the same conductors with electricity, for instance, metals and water; and by being intercepted by the same non-conductors, for instance, glass and sealing-wax. I will not at present trouble you with a detail of our experiments, especially as we are daily advancing in them, but only observe, that, having discovered the back and breast of the animal (I speak in particular of that assemblage of pliant cylinders described by Lorenzini, running perpendicularly from the skin of the back to that of the breast,) to be in different states of electricity, we have been able to convey his shocks, though they were very small, through a circuit of four persons all feeling them; likewise through a considerable length of wire held by two insulated persons, one touching his lower surface and the other his upper.

When the wire was exchanged for glass or sealing-wax, no effect could be perceived. When resumed, the two persons were again sensible of the shock. These experiments have been varied and repeated times without number, by which the choice of conductors is beyond a doubt determined to be the same in the Torpedo as in the Leyden phial. The sensations they occasion, likewise, in the human frame are precisely similar. There is not an *engourdissement* or *fourmillement* of the Torpedo, that we do not exactly imitate with the phial by means of Mr. Lane's electrometer.

We have not yet perceived any spark or noise to

Franklin, as to the mode of conducting his experiments. (See Vol. V. pp. 412, 413.) Mr. Walsh's discoveries were deemed of sufficient importance to give him a claim to the Copley Medal, which was conferred on him by the Royal Society, November 30th, 1774. See Sir John Pringle's *Six Discourses*, p. 45. — Editor.

accompany the shock, nor the pith balls to be ever affected. Indeed all our trials have been on very feeble subjects, whose shock was seldom sensible beyond the touching finger. I remember but one in at least two hundred, that I myself must have taken, to have extended above the elbow. Perhaps L'Isle de Ré, which we are about to visit, may furnish us with Torpedos, fresher taken and of more vigor, by which a farther insight into these matters may be obtained. Our experiments have been chiefly in the air, where the animal was more open to our examination than in water. It is a singularity, that the Torpedo when insulated should be able to give us, insulated likewise, thirty, forty, or fifty successive shocks from nearly the same part, and these with very little diminution in their strength; and indeed they were all minute. Each effort to give a shock is conveniently accompanied with a suppression of his eyes, by which even his attempts to give it to non-conductors could be ascertained. The animal, with respect to the rest of his body, is in a great degree motionless, but not wholly so.

You will please to acquaint Dr. Bancroft of our having verified his prediction concerning the Torpedo, and make any other communications of this matter you may judge proper. Here, I shall be glad to excite as far as I am able both electricians and naturalists to push their inquiries concerning this extraordinary animal, while the summer affords them the opportunity.

I am, with the truest sentiments of esteem and regard, dear Sir, &c.

JOHN WALSH.

FROM JOHN BAPTIST TODERINI TO B. FRANKLIN.

Giving an Account of a Work published by him, entitled "Filosofia Frankliniana."

TRANSLATION.

Forli, 15 August, 1772.

SIR,

In submitting to you my little work, entitled *Filosofia Frankliniana sopra le Punte Preservatrici del Fulmine, &c.*, I place it in the hands of a philosopher, known to the public as one of the most distinguished, whose happy inventions have so greatly advanced the progress of Natural Philosophy. Your great mind, truly English, and your enlightened views, have been directed to the subject of protecting houses and other edifices from lightning.

Availing myself of your ideas, I have thought of extending these preservatives to vessels and to magazines of powder. I propose every thing as derived from you, and as such I acknowledge it. I am very desirous of knowing your opinion of this little work. I shall profit by your suggestions. It will give me great pleasure, Sir, if you have time, and will have the goodness to inform me whether in America, or in England, these defences against lightning have ever been used for vessels; or, whether you, Mr. Watson, or others have invented such preservatives. Pray excuse my inquisitiveness, and if you will favor me with an answer, you can leave it with Mr. Berlendis, Venetian Minister at London.

I am, with great respect, Sir,

Your most obedient, &c.

JOHN BAPTIST TODERINI,
of the Society of Jesus.

ANSWER TO M. DUBOURG'S QUERIES RESPECTING THE ARMONICA.*

London, 8 December, 1772.

When the glasses are ranged on the horizontal spindle, or, to make use of your expression, *enfilés*, and each one is definitely fixed in its place, the whole of the largest glass appears, at the extremity to the left; the following one, nearly enclosed in the preceding one, shows only about an inch of its border, which advances so much further than the edge of the larger glass; and so, in succession, each glass exceeds the one containing it, leaving by this placement an uncovered border on which the fingers may be applied. The glasses do not touch one another, but they are so near as not to admit a finger to pass between them; so that the interior border is not susceptible of being rubbed.

The finger is to be applied flat on the borders of the largest glasses, and on the borders of the smaller; but in part on the borders, and in part on the edges, of the glasses of an intermediate size. Nothing but experience can instruct with respect to this manutation, (*fingering*,) because the different-sized glasses require to be touched differently, some nearer the edge, and others farther from it. A few hours' exercise will teach this. B. F.

* See a description of the *Armonica* above, pp. 245–250; and also plate X.

Some Directions for drawing out the Tones from the Glasses of the Armonica.

Before you sit down to play, the fingers should be well washed with soap and water, and the soap well rinsed off.

The glasses must be always kept perfectly clean from the least greasiness; therefore suffer nobody to touch them with unwashed hands; for even the common slight natural greasiness of the skin rubbed on them will prevent their sounding for a long time.

You must be provided with a bottle of rain water, (spring water is generally too hard and produces a harsh tone,) and a middling sponge in a little slop-bowl, in which you must keep so much of the water that the sponge may be always very wet.

In a teacup keep also ready some fine scraped chalk, free from grit, to be used on occasion.

The fingers when you begin to play should not only be wet on the surface, but the skin a little soaked, which is readily done by pressing them hard a few times in the sponge.

The first thing after setting the glasses in motion is to pass the sponge slowly along from the biggest glass to the smallest, suffering it to rest on each glass during at least one revolution of the glasses, whereby they will all be made moderately wet. If too much water is left on them, they will not sound so readily.

If the instrument is near a window, let the window be shut or the curtain drawn, as wind or sunshine on the glasses dries them too fast.

When these particulars are all attended to, and the directions observed, the tone comes forth finely with the slightest pressure of the fingers imaginable, and

you swell it at pleasure by adding a little more pressure, no instrument affording more shades, if one may so speak, of the Forte Piano.

One wetting with the sponge will serve for a piece of music twice as long as Handel's Water-piece, unless the air be uncommonly drying.

But a number of thin slices of sponge, placed side by side, and their ends held fast between two stripes of wood, like rulers, of a length equal to the glasses, and placed so that the loose ends of the sponges may touch the glasses behind, and by that means keep them constantly wet, is very convenient where one proposes to play a long time. The sponges being properly wetted will supply the glasses sufficiently a whole evening, and touching the glasses lightly do not in the least hurt the sound.

The powder of chalk is useful two ways.

Fingers, after much playing, sometimes begin to draw out a tone less smooth and soft, and you feel as well as hear a small degree of sharpness. In this case, if you dip the ends of your wet fingers in the chalk, so as to take up a little, and rub the same well on the skin, it will immediately recover the smoothness of tone desired. And, if the glasses have been sullied by handling, or the fingers not being just washed have some little greasiness on them, so that the sounds cannot easily be produced, chalk so used will clean both glasses and fingers, and the sounds will come out to your wish.

A little practice will make all this familiar; and you will also find by trials what part of the fingers most readily produces the sound from particular glasses, and whether they require to be touched on the edge chiefly, or a little more on the side; as different glasses require a different touch, some pretty full on the flat side of

the brim, to bring out the best tone, others more on the edge, and some of the largest may need the touch of two fingers at once.

Of the Stilling of Waves by Means of Oil. Extracted from sundry Letters between Benjamin Franklin, L. L. D., F. R. S., William Brownrigg, M. D., F. R. S., and the Reverend Mr. Farish.

READ AT THE ROYAL SOCIETY, JUNE 2d, 1774.

Extract of a Letter from Dr. Brownrigg to Dr. Franklin, dated Ormathwaite, 27 January, 1773.

By the enclosed from an old friend, a worthy clergyman at Carlisle, whose great learning and extensive knowledge in most sciences would have more distinguished him, had he been placed in a more conspicuous point of view, you will find, that he had heard of your experiment on Derwent Lake, and has thrown together what he could collect on that subject; to which I have subjoined one experiment from the relation of another gentleman.

Extract of a Letter from the Reverend Mr. Farish, to Dr. Brownrigg.

"I some time ago met with Mr. Dun, who surprised me with an account of an experiment you had tried upon the Derwent Water, in company with Sir John Pringle and Dr. Franklin. According to his representation, the water, which had been in great agitation before, was instantly calmed upon pouring in only a very small quantity of oil, and that to so great a distance round the boat as seemed incredible. I have

since had the same accounts from others, but I suspect all of a little exaggeration. Pliny mentions this property of oil as known particularly to the divers, who made use of it in his days, in order to have a more steady light at the bottom.* The sailors, I have been told, have observed something of the same kind in our days, that the water is always remarkably smoother, in the wake of a ship that has been newly tallowed, than it is in one that is foul. Mr. Pennant also mentions an observation of the like nature made by the seal-catchers in Scotland. (*Brit. Zool.* Vol. IV., *Article* SEAL.) When these animals are devouring a very oily fish, which they always do under water, the waves above are observed to be remarkably smooth, and by this mark the fishermen know where to look for them. Old Pliny does not usually meet with all the credit I am inclined to think he deserves. I shall be glad to have an authentic account of the Keswick experiment; and, if it comes up to the representations that have been made of it, I shall not much hesitate to believe the old gentleman in another more wonderful phenomenon he relates, of stilling a tempest only by throwing up a little vinegar into the air."

* Sir Gilfred Lawson, who served long in the army at Gibraltar, assures me, that the fishermen in that place are accustomed to pour a little oil on the sea, in order to still its motion, that they may be enabled to see the oysters lying at its bottom; which are there very large, and which they take up with a proper instrument. This Sir Gilfred had often seen there performed, and said the same was practised on other parts of the Spanish coast.—*Note by Dr. Brownrigg.*

Extract of a Letter to Dr. Brownrigg from Dr. Franklin, dated London, 7 November, 1773.

DEAR SIR,

I thank you for the remarks of your learned friend at Carlisle. I had, when a youth, read and smiled at Pliny's account of a practice among the seamen of his time, to still the waves in a storm by pouring oil into the sea; which he mentions, as well as the use made of oil by the divers; but the stilling a tempest by throwing vinegar into the air had escaped me. I think with your friend, that it has been of late too much the mode to slight the learning of the ancients. The learned, too, are apt to slight too much the knowledge of the vulgar. The cooling by evaporation was long an instance of the latter. This art of smoothing the waves by oil is an instance of both.

Perhaps you may not dislike to have an account of all I have heard, and learnt, and done in this way. Take it if you please as follows.

In 1757, being at sea in a fleet of ninety-six sail bound against Louisbourg, I observed the wakes of two of the ships to be remarkably smooth, while all the others were ruffled by the wind, which blew fresh. Being puzzled with the differing appearance, I at last pointed it out to our captain, and asked him the meaning of it. "The cooks," says he, "have, I suppose, been just emptying their greasy water through the scuppers, which has greased the sides of those ships a little;" and this answer he gave me with an air of some little contempt, as to a person ignorant of what everybody else knew. In my own mind I at first slighted his solution, though I was not able to think of another; but recollecting what I had formerly read in Pliny, I resolved to make some experiment of the effect of oil on water. when I should have opportunity.

Afterwards being again at sea in 1762, I first observed the wonderful quietness of oil on agitated water, in the swinging glass lamp I made to hang up in the cabin, as described in my printed papers.* This I was continually looking at and considering, as an appearance to me inexplicable. An old sea captain, then a passenger with me, thought little of it, supposing it an effect of the same kind with that of oil put on water to smooth it, which he said was a practice of the Bermudians when they would strike fish, which they could not see, if the surface of the water was ruffled by the wind. This practice I had never before heard of, and was obliged to him for the information; though I thought him mistaken as to the sameness of the experiment, the operations being different as well as the effects. In one case, the water is smooth till the oil is put on, and then becomes agitated. In the other it is agitated before the oil is applied, and then becomes smooth. The same gentleman told me, he had heard it was a practice with the fishermen of Lisbon when about to return into the river (if they saw before them too great a surf upon the bar, which they apprehended might fill their boats in passing) to empty a bottle or two of oil into the sea, which would suppress the breakers, and allow them to pass safely. A confirmation of this I have not since had an opportunity of obtaining; but discoursing of it with another person, who had often been in the Mediterranean, I was informed, that the divers there, who, when under water in their business, need light, which the curling of the surface interrupts by the refractions of so many little waves, let a small quantity of oil now and then out of

* See the letter above, (p. 253) to Dr. John Pringle, dated December 1st, 1762.—Editor.

their mouths, which rising to the surface smooths it, and permits the light to come down to them. All these informations I at times revolved in my mind, and wondered to find no mention of them in our books of experimental philosophy.

At length being at Clapham, where there is, on the common, a large pond, which I observed one day to be very rough with the wind, I fetched out a cruet of oil, and dropped a little of it on the water. I saw it spread itself with surprising swiftness upon the surface; but the effect of smoothing the waves was not produced; for I had applied it first on the leeward side of the pond, where the waves were largest, and the wind drove my oil back upon the shore. I then went to the windward side where they began to form; and there the oil, though not more than a tea spoonful, produced an instant calm over a space several yards square, which spread amazingly, and extended itself gradually till it reached the lee side, making all that quarter of the pond, perhaps half an acre, as smooth as a looking-glass.

After this I contrived to take with me, whenever I went into the country, a little oil in the upper hollow joint of my bamboo cane, with which I might repeat the experiment as opportunity should offer, and I found it constantly to succeed.

In these experiments, one circumstance struck me with particular surprise. This was the sudden, wide, and forcible spreading of a drop of oil on the face of the water, which I do not know that anybody has hitherto considered. If a drop of oil is put on a highly polished marble table, or on a looking-glass that lies horizontally, the drop remains in its place, spreading very little. But, when put on water, it spreads instantly many feet round, becoming so thin as to pro-

duce the prismatic colors, for a considerable space, and beyond them so much thinner as to be invisible, except in its effect of smoothing the waves at a much greater distance. It seems as if a mutual repulsion between its particles took place as soon as it touched the water, and a repulsion so strong as to act on other bodies swimming on the surface, as straw, leaves, chips, &c. forcing them to recede every way from the drop, as from a centre, leaving a large, clear space. The quantity of this force, and the distance to which it will operate, I have not yet ascertained; but I think it a curious inquiry, and I wish to understand whence it arises.

In our journey to the North, when we had the pleasure of seeing you at Ormathwaite, we visited the celebrated Mr. Smeaton, near Leeds. Being about to show him the smoothing experiment on a little pond near his house, an ingenious pupil of his, Mr. Jessop, then present, told us of an odd appearance on that pond, which had lately occurred to him. He was about to clean a little cup in which he kept oil, and he threw upon the water some flies that had been drowned in the oil. These flies presently began to move, and turned round on the water very rapidly, as if they were vigorously alive, though on examination he found they were not so. I immediately concluded that the motion was occasioned by the power of the repulsion above mentioned, and that the oil issuing gradually from the spungy body of the fly continued the motion. He found some more flies drowned in oil, with which the experiment was repeated before us. To show that it was not any effect of life recovered by the flies, I imitated it by little bits of oiled chips and paper, cut in the form of a comma, of the size of a common fly; when the stream of repelling particles issuing from the point

made the comma turn round the contrary way. This is not a chamber experiment; for it cannot be well repeated in a bowl or dish of water on a table. A considerable surface of water is necessary to give room for the expansion of a small quantity of oil. In a dish of water, if the smallest drop of oil be let fall in the middle, the whole surface is presently covered with a thin greasy film proceeding from the drop; but as soon as that film has reached the sides of the dish, no more will issue from the drop, but it remains in the form of oil, the sides of the dish putting a stop to its dissipation by prohibiting the farther expansion of the film.

Our friend Sir John Pringle, being soon after in Scotland, learned there, that those employed in the herring fishery could at a distance see where the shoals of herrings were, by the smoothness of the water over them, which might possibly be occasioned, he thought, by some oiliness proceeding from their bodies.

A gentleman from Rhode Island told me, it had been remarked, that the harbour of Newport was ever smooth while any whaling vessels were in it; which probably arose from hence, that the blubber which they sometimes bring loose in the hold, or the leakage of their barrels, might afford some oil, to mix with that water, which from time to time they pump out, to keep their vessel free, and that some oil might spread over the surface of the water in the harbour, and prevent the forming of any waves.

This prevention I would thus endeavour to explain.

There seems to be no natural repulsion between water and air, such as to keep them from coming into contact with each other. Hence we find a quantity of air in water; and if we extract it by means of the air-pump, the same water, again exposed to the air, will soon imbibe an equal quantity.

Therefore air in motion, which is wind, in passing over the smooth surface of water, may rub, as it were, upon that surface, and raise it into wrinkles, which, if the wind continues, are the elements of future waves.

The smallest wave once raised does not immediately subside, and leave the neighbouring water quiet; but in subsiding raises nearly as much of the water next to it, the friction of the parts making little difference. Thus a stone dropped in a pool raises first a single wave round itself; and leaves it, by sinking to the bottom; but that first wave subsiding raises a second, the second a third, and so on in circles to a great extent.

A small power continually operating will produce a great action. A finger applied to a weighty suspended bell can at first move it but little; if repeatedly applied, though with no greater strength, the motion increases till the bell swings to its utmost height, and with a force that cannot be resisted by the whole strength of the arm and body. Thus the small first-raised waves, being continually acted upon by the wind, are, though the wind does not increase in strength, continually increased in magnitude, rising higher and extending their basis, so as to include a vast mass of water in each wave, which in its motion acts with great violence.

But if there be a mutual repulsion between the particles of oil, and no attraction between oil and water, oil dropped on water will not be held together by adhesion to the spot whereon it falls; it will not be imbibed by the water; it will be at liberty to expand itself; and it will spread on a surface that, besides being smooth to the most perfect degree of polish, prevents, perhaps by repelling the oil, all immediate contact, keeping it at a minute distance from itself; and the expansion will continue till the mutual repulsion between the particles of the oil is weakened and reduced to nothing by their distance.

Now I imagine that the wind, blowing over water thus covered with a film of oil, cannot easily *catch* upon it, so as to raise the first wrinkles, but slides over it, and leaves it smooth as it finds it. It moves a little the oil indeed, which being between it and the water, serves it to slide with, and prevents friction, as oil does between those parts of a machine, that would otherwise rub hard together. Hence the oil dropped on the windward side of a pond proceeds gradually to leeward, as may be seen by the smoothness it carries with it, quite to the opposite side. For the wind being thus prevented from raising the first wrinkles, that I call the elements of waves, cannot produce waves, which are to be made by continually acting upon, and enlarging those elements, and thus the whole pond is calmed.

Totally therefore we might suppress the waves in any required place, if we could come at the windward place where they take their rise. This in the ocean can seldom if ever be done. But perhaps something may be done on particular occasions, to moderate the violence of the waves when we are in the midst of them, and prevent their breaking where that would be inconvenient.

For, when the wind blows fresh, there are continually rising on the back of every great wave a number of small ones, which roughen its surface, and give the wind hold, as it were, to push it with greater force. This hold is diminished, by preventing the generation of those small ones. And possibly too, when a wave's surface is oiled, the wind, in passing over it, may rather in some degree press it down, and contribute to prevent its rising again, instead of promoting it.

This, as mere conjecture, would have little weight, if the apparent effects of pouring oil into the midst of

waves were not considerable, and as yet not otherwise accounted for.

When the wind blows so fresh, as that the waves are not sufficiently quick in obeying its impulse, their tops being thinner and lighter are pushed forward, broken, and turned over in a white foam. Common waves lift a vessel without entering it; but these when large sometimes break above and pour over it, doing great damage.

That this effect might in any degree be prevented, or the height and violence of waves in the sea moderated, we had no certain account; Pliny's authority for the practice of seamen in his time being slighted. But discoursing lately on this subject with his Excellency Count Bentinck, of Holland, his son the Honorable Captain Bentinck, and the learned Professor Allemand, (to all whom I showed the experiment of smoothing in a windy day the large piece of water at the head of the Green Park,) a letter was mentioned, which had been received by the Count from Batavia, relative to the saving of a Dutch ship in a storm by pouring oil into the sea. I much desired to see that letter, and a copy of it was promised me, which I afterward received.

Extract of a Letter from Mr. Tengnagel to Count Bentinck, dated at Batavia, 5 January, 1770.

"Near the islands Paul and Amsterdam, we met with a storm, which had nothing particular in it worthy of being communicated to you, except that the captain found himself obliged for greater safety in wearing the ship, to pour oil into the sea, to prevent the waves breaking over her, which had an excellent effect, and succeeded in preserving us. As he poured out but a

little at a time, the East India Company owes perhaps its ship to only six demi-ames of oil-olive. I was present upon deck when this was done; and I should not have mentioned this circumstance to you, but that we have found people here so prejudiced against the experiment, as to make it necessary for the officers on board and myself to give a certificate of the truth on this head, of which we made no difficulty."

On this occasion, I mentioned to Captain Bentinck a thought which had occurred to me in reading the voyages of our late circumnavigators, particularly where accounts are given of pleasant and fertile islands which they much desired to land upon, when sickness made it more necessary, but could not effect a landing through a violent surf breaking on the shore, which rendered it impracticable. My idea was, that possibly by sailing to and fro at some distance from such lee-shore, continually pouring oil into the sea, the waves might be so much depressed, and lessened before they reached the shore, as to abate the height and violence of the surf, and permit a landing; which, in such circumstances, was a point of sufficient importance to justify the expense of the oil that might be requisite for the purpose. That gentleman, who is ever ready to promote what may be of public utility, though his own ingenious inventions have not always met with the countenance they merited, was so obliging as to invite me to Portsmouth, where an opportunity would probably offer, in the course of a few days, of making the experiment on some of the shores about Spithead, in which he kindly proposed to accompany me, and to give assistance with such boats as might be necessary. Accordingly, about the middle of October last, I went with some friends to Portsmouth; and a day of wind

happening, which made a lee-shore between Haslar hospital and the point near Jillkecker, we went from the Centaur with the longboat and barge towards that shore. Our disposition was this; the longboat was anchored about a quarter of a mile from the shore; part of the company were landed behind the point (a place more sheltered from the sea) who came round and placed themselves opposite to the longboat, where they might observe the surf, and note if any change occurred in it upon using the oil. Another party, in the barge, plied to windward of the longboat, as far from her as she was from the shore, making trips of about half a mile each, pouring oil continually out of a large stone bottle, through a hole in the cork, some-

Ca[illegible] what bigger than a goose-quill. The experiment had not[illegible] in the main point, the success we wished, for no material [illegible] difference was observed in the height or force of the surf upon the shore; but those who were in the longboat could observe a tract of smoothed water, the whole of the distance in which the barge poured the oil, and gradually spreading in breadth towards the long-boat. I call it smoothed, not that it was laid level; but because, though the swell continued, its surface was not roughened by the wrinkles, or smaller waves, before mentioned; and none or very few white caps (or waves whose tops turn over in foam) appeared in that whole space, though to windward and leeward of it there were plenty; and a wherry, that came round the point under sail, in her way to Portsmouth, seemed to turn into that tract of choice, and to use it from end to end, as a piece of turnpike road.

It may be of use to relate the circumstances of an experiment that does not succeed, since they may give hints of amendment in future trials; it is therefore I have been thus particular. I shall only add what I

apprehend may have been the reason of our disappointment.

I conceive, that the operation of oil on water is, first, to prevent the raising of new waves by the wind; and, secondly, to prevent its pushing those before raised with such force, and consequently their continuance of the same repeated height, as they would have done, if their surface were not oiled. But oil will not prevent waves being raised by another power, by a stone, for instance, falling into a still pool; for they then rise by the mechanical impulse of the stone, which the greasiness on the surrounding water cannot lessen or prevent, as it can prevent the winds catching the surface and raising it into waves. Now waves once raised, whether by the wind or any other power, have the same mechanical operation, by which they continue to rise and fall, as a *pendulum* will continue to swing a long time after the force ceases to act by which the motion was first produced; that motion will, however, cease in time; but time is necessary. Therefore, though oil spread on an agitated sea may weaken the push of the wind on those waves whose surfaces are covered by it, and so, by receiving less fresh impulse, they may gradually subside; yet a considerable time, or a distance through which they will take time to move, may be necessary to make the effect sensible on any shore in a diminution of the surf; for we know, that, when wind ceases suddenly, the waves it has raised do not as suddenly subside, but settle gradually, and are not quite down till after the wind has ceased. So, though we should, by oiling them, take off the effect of wind on waves already raised, it is not to be expected that those waves should be instantly levelled. The motion they have received will, for some time, continue; and, if the shore is not far distant, they arrive there so soon, that

their effect upon it will not be visibly diminished. Possibly, therefore, if we had begun our operations at a greater distance, the effect might have been more sensible. And perhaps we did not pour oil in sufficient quantity. Future experiments may determine this.

I was, however, greatly obliged to Captain Bentinck, for the cheerful and ready aids he gave me; and I ought not to omit mentioning Mr. Banks, Dr. Solander, General Carnoc, and Dr. Blagden, who all assisted at the experiment, during that blustering, unpleasant day, with a patience and activity that could only be inspired by a zeal for the improvement of knowledge, such especially as might possibly be of use to men in situations of distress.

I would wish you to communicate this to your ingenious friend, Mr. Farish, with my respects; and believe me to be, with sincere esteem, dear Sir,

Your most obedient humble servant,

B. FRANKLIN.

TO HUMPHRY MARSHALL.

On the Spots in the Sun; Dr. Wilson's New Hypothesis.

London, 14 February, 1773.

SIR,

A considerable time after its arrival, I received the box of seeds you sent me the beginning of last year, with your observations on spots of the sun. The seeds I distributed among some of my friends who are curious; accept my thankful acknowledgments for them. The observations I communicated to our astronomers of the Royal Society, who are much pleased with them,

and hand them about from one to another; so that I have had little opportunity of examining them myself, they not being yet returned to me.

Here are various opinions about the solar spots. Some think them vast clouds of smoke and soot arising from the consuming fuel on the surface, which at length take fire again on their edges, consuming and daily diminishing till they totally disappear. Others think them spots of the surface, in which the fire has been extinguished, and which by degrees is rekindled. It is however remarkable, that, though large spots are seen gradually to become small ones, no one has observed a small spot gradually become a large one; at least I do not remember to have met with such an observation. If this be so, it should seem they are suddenly formed of their full size; and perhaps, if there were more such constant and diligent observers as you, some might happen to be observing at the instant such a spot was formed, when the appearances might give some ground of conjecture by what means they were formed.

The professor of astronomy at Glasgow, Dr. Wilson, has a new hypothesis. It is this; that the sun is a globe of solid matter, all combustible, perhaps, but whose surface only is actually on fire to a certain depth, and all below that depth unkindled, like a log of wood, whose surface to half an inch deep may be burning coal, while all within remains wood. Then he supposes, by some explosion similar to our earthquakes, the burning part may be blown away from a particular district, leaving bare the unkindled part below, which then appears a spot, and only lessens as the fluid burning matter by degrees flows in upon it on all sides, and at last covers or rekindles it.

He founds this opinion on certain appearances of

the edges of the spots as they turn under the sun's disk, or emerge again on the other side; for, if there are such hollows in the sun's face as he supposes, and the bright border round their edges be the fluid burning matter flowing down the banks into the hollow, it will follow, that, while a spot is in the middle of the sun's disk, the eye looking directly upon the whole, may discern that border all round; but when the hollow is moved round to near the edge of the disk, then, though the eye which now views it aslant can see full the farthest bank, yet that which is nearest is hidden, and not to be distinguished; and when the same spot comes to emerge again on the other side of the sun, the bank which before was visible is now concealed, and that concealed which before was visible, gradually changing, however, till the spot reaches the middle of the disk, when the bank all round may be seen as before. Perhaps your telescope may be scarce strong enough to observe this. If it is, I wish to know whether you find the same appearances. When your observations are returned to me, and I have considered them, I shall lodge them among the papers of the Society, and let you know their sentiments.

With great esteem and regard, I am, &c.

B. FRANKLIN.*

* In the *Philosophical Transactions* (Vol. LXIV. p. 194) is a letter from Humphry Marshall to Dr. Franklin on solar spots. — EDITOR.

FROM M. DE SAUSSURE TO B. FRANKLIN.*

Conductors of Lightning. — Project of the Royal Society to ascertain the Attraction of Mountains. — Volcanic Eruptions. — Experiments on the Torpedo.

TRANSLATION.

Naples, 23 February, 1773.

SIR,

I have received with the greatest pleasure the two letters, which you did me the honor to write to me; the one of October 8th, the other of December 1st. As they were both addressed to me at Geneva, and as I left that place in the beginning of October to come to Italy to pass the winter, they reached me very late, and I have thus been debarred the privilege of showing you by a prompt reply, how much I feel flattered by the honor of your correspondence. The letter on the action of pointed conductors, and the accompanying *Essay*, contain experiments and reasonings perfectly conclusive, and which leave no doubt as to the utility of these ingenious preservatives.†

If I had been acquainted with these new experiments, I should have made use of them with great advantage, in a short apologetic memoir, which I published in October, 1771, for the information of some people, who were terrified at a conductor, which I had erected at Geneva before the house I lived in. This Memoir, however, met with the desired success. It

* M. De Saussure was the well-known professor at Geneva, celebrated throughout Europe for his philosophical writings, and particularly for his interesting account of his ascent of Mont Blanc. — EDITOR.

† Franklin's letters to M. de Saussure have not been preserved. The papers here alluded to were probably in part those relating to the powder magazines at Purfleet. See Vol. V. pp. 427–445. — EDITOR.

reassured everybody, and I had the pleasure of watching the electricity from the clouds during the whole course of the last summer. Several persons even followed this example, and raised conductors either upon their houses or before them. M. de Voltaire was one of the first. He does the same justice to your theory, that he did to that of the immortal Newton.

The project of the Royal Society is well worthy of the zeal of that illustrious body for the advancement of useful knowledge; and I should be much pleased, if I could in any way aid them in the execution of this project.* Had I been at Geneva, I should have made it my duty and pleasure to take a journey to the mountains in the neighbourhood, to ascertain with precision the dimensions of the mountains and valleys, which I thought best suited for the execution of this design. I do not believe, however, that, among those with which I am acquainted, there is any place exactly suited to give certain information on the subject, to which their researches are directed. In the Jura, there is no summit sufficiently high, since the Dole, the mountain which rises highest above the level of our lake, does not reach seven hundred *toises* above this level.

Then it must be considered that the Jura, as well as the Alps, form continued chains of mountains, all

* The Royal Society had recently engaged in the project of ascertaining the lateral attraction of mountains, with the view of determining the mean density of the earth upon the Newtonian theory of gravitation. On this subject, it would seem, Dr. Franklin had written to request the aid of M. de Saussure, who had bestowed much time and attention in observing the geological structure and formation of the mountains of the Alps. The object of the Society was finally attained by Dr. Maskelyne, the Astronomer Royal, who made a series of observations on the Schihallion Mountain, in Perthshire, which were considered conclusive. He received the Copley Medal from the Royal Society, for this successful enterprise, on the 30th of November, 1775. See Sir John Pringle's *Six Discourses*, p. 93. — Editor.

connected together, or, at least, situated at very short distances from each other. There is no single mountain, or, at least, I know of none, of sufficient height. You often find deep valleys, surrounded by high mountains, but behind these mountains are other valleys and other mountains, so that the deviations which might be observed in the plumb-line, would be the complex effect of the combined attractions of all these mountains; and, in order to deduce from them a comparison between the density of the earth and that of these mountains, much labor and many calculations would be required. As far as I am able to judge, it appears to me that some large rock, rising out of the open sea, like the Peak of Teneriffe, would be the most suitable place for this attempt.

The memoir upon this subject, which you did me the honor to send, I have transmitted to Lord Stanhope, at Geneva, that he may confer respecting it with M. de Luc, who, having attended particularly to the height of mountains, in connexion with his observations of the barometer, is the best man in the world to give valuable information on this subject. It is now, doubtless, well known that Signor Beccaria of Turin, who has measured a degree of the meridian at the foot of the Alps, has had an opportunity to observe great deviations of the plumb-line, and would thus be enabled to furnish useful hints to the Royal Society.

I have the pleasure of often seeing here Sir William Hamilton, who has the kindness to take me to the most interesting places in the neighbourhood of Naples, those which establish his theory of volcanoes ancient and modern, and prove that the whole Bay of Naples, from the sea to the Apennines, has been thrown up from the bottom of the sea by subterranean fires, and is thus the product of volcanoes rather than the theatre

of their ravages. We are also much devoted to electricity. The little machine, which Mr. Nairne made for him, is really excellent, and much the best they have ever had in this part of Italy. Sir William Hamilton, knowing that I had the honor of writing to you, has requested me to present his compliments.

I regret extremely that I was not at Geneva to receive M. de Normandy. I should have been delighted to have had this opportunity to prove to you how highly I value your recommendation. If you have any commands for me in Naples, I shall still be able to receive them here, and you may address them to Sir William Hamilton. We propose to try together some experiments on the electricity of the vapours of Vesuvius, although, to say the truth, I regard them merely as conductors, which establish a communication between the earth and the higher regions of the atmosphere.

Sir William Hamilton has also done me the kindness to invite me to witness some experiments he has tried with the Torpedo. These experiments are not decisive, because the fishes we had were small, and gave only slight shocks, but no sign whatever of electricity appeared. We are waiting for some larger ones, in order to continue this investigation, according to the mode which you have yourself marked out.*

Accept my assurances of the high consideration and esteem, with which I have the honor to be,

Sir, &c.

DE SAUSSURE.

* See the rules given by Dr. Franklin on this subject, Vol. V. p. 412. Also a letter from Mr. Walsh above, p. 348.— EDITOR.

FROM JOHN WINTHROP TO B. FRANKLIN.

Dr. Priestley. — Effects of Lightning.

Cambridge, New England, 4 March, 1773.

DEAR SIR,

I received your favor of September 18th. I return you many thanks for Dr. Priestley's piece on impregnating water with fixed air. If this should prove an effectual remedy for the sea-scurvy, it would be indeed a most important discovery. I am extremely concerned to hear that Dr. Priestley is so poorly provided for, while so many are rolling about here in gilt chariots, with very ample stipends. I admire his comprehensive genius, his perspicuity and vigor of composition, his indefatigable application, and his free, independent spirit, and wish it were in my power to do him any kind of service. It would give me great pleasure to see him well settled in America; though indeed I am inclined to think he can prosecute his learned labors to greater advantage in England. A man of his abilities would do honor to any of the colleges. At present there is no vacancy among them; but if there were, I believe, Sir, you judge perfectly right, that his religious principles would hardly be thought orthodox enough. Indeed, I doubt, whether they would do at the Rhode Island College, any more than in the others. That college is entirely in the hands of the Baptists, and intended to continue so, and I never understood that Dr. Priestley was of their persuasion. However, I cannot but hope that his great and just reputation will procure something valuable for him, and adequate to his merit.

I have looked over his treatise of Optics, which you were so good as to present to our library, with great

satisfaction, and met with many articles, especially from the foreign publications, which were new to me. It is indeed a most noble collection of every thing relating to that science.

In my last I ventured to mention a little slip concerning the satellites of Saturn. It would be miraculous, if, in so large a work collected from such a number of books and on such a variety of matters, there should not be many such. I noted the few that occurred to me in the chapters taken from those authors I was most acquainted with, and beg leave to enclose a list of the principal of them. There are not above two or three of them that are of any consequence; however, such as it is, the list is at Dr. Priestley's service, if you think it worth sending to him. It may help to remove a few trifling inaccuracies from that valuable work.

I have enclosed the newspaper you mention, that gave an account of the thunder-storm we had here a few years ago. As you are collecting facts on this subject, I looked over my old Almanacs where I had made some memoranda relating to your admirable lightning bells. I think it would not be worth while to transcribe them all, nor can I collect any thing from them but what is commonly known. In general, it seems that the bells hardly ever ring in the summer without a shower; they sometimes ring when there is no thunder or lightning, but do not always ring when there is. When there is a thunder shower, they generally ring most briskly while the cloud is yet at some distance, and cease as soon as it rains hard. In winter they frequently ring briskly in snow-storms, and twice they have done so after the weather was cleared up, and while the new-fallen snow was driving about with the wind, as you have done me the honor already to publish.

In looking for the newspaper before mentioned, I met with another, which gives an account of damage done by lightning in some places in Connecticut in 1771. As perhaps you have not seen it, I enclose it with the other; also, a letter sent me with another account. In my Almanacs I found also a few minutes relating to some uncommon appearances of the Aurora Borealis. I do not know that they can be of any use, but if they will afford you the least amusement I will readily transcribe them.

In addition to my newspaper account, I would mention that besides the strokes of lightning on the College and the elm tree, July 2d, 1768, there was another discharge that afternoon on a cornfield, at a little distance from the College towards the southeast. It spoiled the corn,* which was of some height, in a circle of about twenty feet diameter. That near the centre was burnt down to the roots, as I was informed by the owner. I did not hear of it till some days after, and when I saw the place it had been replanted with cabbages. The corn near the circumference of the circle was only scorched, and I saw the leaves withered and drooping. The place struck was about midway between a tree on one side, and the well-pole and chimney of the house on the other, and, as I judge, about eighty feet distant from each; and there was nothing near so high on the other sides for a considerable distance. Hence, their protection did not extend eighty feet. If a person had been standing in that corn, I suppose there is no doubt that he would have been killed. And therefore a person in the midst of an open plain is by no means secure from the stroke

* Indian corn, or maize, which is most commonly planted in this neighbourhood.—*Note by Dr. Franklin.*

of lightning. The best security seems to be to have something high, as a tree for example, near him, but not too near; perhaps from thirty or forty to ten or fifteen feet, or rather to be near two such trees.

I am, &c.

JOHN WINTHROP.

TO M. DUBOURG.*

On the Causes of Colds.

10 March, 1773.

I shall not attempt to explain why damp clothes occasion colds, rather than wet ones, because I doubt the fact; I imagine that neither the one nor the other contribute to this effect, and that the causes of colds are totally independent of wet and even of cold. I propose writing a short paper on this subject, the first moment of leisure I have at my disposal. In the mean time I can only say, that, having some suspicions that the common notion, which attributes to cold the property of stopping the pores and obstructing perspiration, was ill founded, I engaged a young physician, who is making some experiments with Sanctorius's balance, to estimate the different proportions of his perspiration, when remaining one hour quite naked, and another warmly clothed. He pursued the experiment in this alternate manner for eight hours successively, and found his perspiration almost double during those hours in which he was naked.

B. FRANKLIN.

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* Translated from M. Dubourg's edition of Franklin's Works, Vol. II. p. 311. — EDITOR.

TO M. LE ROY.

Mr. Wilson's Opposition to Pointed Conductors. — M. Le Roy Chosen a Member of the American Philosophical Society.

London, 30 March, 1773.

DEAR SIR,

You punish my delay of writing to you very properly, by not writing to me. It is long since I have had the pleasure of hearing from you. But it is my fault, and I must for my own sake write to you oftener, though I have little to say, or you will quite forget me.

I thank you for your advice to send an English copy of my writings to the Academy, and shall do it as soon as the new edition now in hand here is finished.

I am glad you see some weight in the experiments I sent you concerning pointed rods. Mr. Wilson is grown angry, that his advice was not followed in making them blunt for the public magazines of gunpowder, and has published a pamphlet reflecting on the Royal Society, the committee, and myself, with some asperity, and endeavouring to alarm the city with the supposed danger of pointed rods drawing the lightning into them, and blowing them up. I find it is expected from me that I make some answer to it, and I shall do so, though I have an extreme aversion to public altercation on philosophic points, and have never yet disputed with any one, who thought fit to attack my opinions. I am obliged to you for the experiment of the point and ring.

There is no being sure of any thing before it happens; but, considering the weight of your reputation, I think there is little reason to doubt the success of your friends' endeavours to procure from our Society here

the honor of adding you to their number at the next election. In the mean time will you for my sake confer the same kind of honor on our young Society at Philadelphia. When I found that our first volume of American Transactions was favorably received in Europe, and had procured us some reputation, I took the liberty of nominating you for a member, and you were accordingly chosen at a full meeting in Philadelphia on the 15th of January last. I sent a copy of that volume to the Academy of Sciences at Paris, when it first came out, but I do not remember to have heard that they ever received it. I think it was Mr. Magalhaens,* who undertook to convey it. If it miscarried, I will send another, and by the first opportunity one for yourself.

Two ships are now fitting out here by the Admiralty, at the request of the Royal Society, to make a voyage to the north pole, or to go as near to it as the ice will permit. If they return safe we shall probably obtain some new geographical knowledge, and some addition to natural history.

With the greatest esteem and respect, I am, &c.

B. FRANKLIN.

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* John Hyacinth de Magalhaens, a Portuguese by birth, who resided a large part of his life in England. His name frequently occurs in Franklin's letters. He is said to have been "an able linguist, and well versed in chemistry and natural philosophy," and to have published respectable treatises on mineralogy, and some other branches of science. He was a member of the Royal Society. This is the same person (whose name is sometimes printed *Magellan*) that gave to the American Philosophical Society a donation of two hundred guineas, which was to be invested in a secure fund, and the interest disposed of annually in premiums to the author of the best discovery, or most useful invention, relating to navigation, astronomy, or natural philosophy. — EDITOR.

TO M. DUBOURG.

Observations on the prevailing Doctrines of Life and Death.

Your observations on the causes of death, and the experiments which you propose for recalling to life those who appear to be killed by lightning, demonstrate equally your sagacity and your humanity. It appears, that the doctrines of life and death, in general, are yet but little understood.

A toad buried in sand will live, it is said, till the sand becomes petrified; and then, being enclosed in the stone, it may still live for we know not how many ages. The facts which are cited in support of this opinion are too numerous, and too circumstantial, not to deserve a certain degree of credit. As we are accustomed to see all the animals, with which we are acquainted, eat and drink, it appears to us difficult to conceive, how a toad can be supported in such a dungeon; but if we reflect, that the necessity of nourishment, which animals experience in their ordinary state, proceeds from the continual waste of their substance by perspiration, it will appear less incredible, that some animals in a torpid state, perspiring less because they use no exercise, should have less need of aliment; and that others, which are covered with scales or shells, which stop perspiration, such as land and sea turtles, serpents, and some species of fish, should be able to subsist a considerable time without any nourishment whatever. A plant, with its flowers, fades and dies immediately, if exposed to the air without having its root immersed in

* Translated from M. Dubourg's edition of Franklin's Works, Vol. I. p. 327. It is without date, but the letter to which it is an answer is dated April 15th, 1773. — EDITOR.

a humid soil, from which it may draw a sufficient quantity of moisture to supply that which exhales from its substance and is carried off continually by the air. Perhaps, however, if it were buried in quicksilver, it might preserve for a considerable space of time its vegetable life, its smell, and color. If this be the case, it might prove a commodious method of transporting from distant countries those delicate plants, which are unable to sustain the inclemency of the weather at sea, and which require particular care and attention. I have seen an instance of common flies preserved in a manner somewhat similar. They had been drowned in Madeira wine, apparently about the time when it was bottled in Virginia, to be sent hither (to London). At the opening of one of the bottles, at the house of a friend where I then was, three drowned flies fell into the first glass that was filled. Having heard it remarked, that drowned flies were capable of being revived by the rays of the sun, I proposed making the experiment upon these ; they were therefore exposed to the sun upon a sieve, which had been employed to strain them out of the wine. In less than three hours, two of them began by degrees to recover life. They commenced by some convulsive motions of the thighs, and at length they raised themselves upon their legs, wiped their eyes with their fore feet, beat and brushed their wings with their hind feet, and soon after began to fly, finding themselves in Old England, without knowing how they came thither. The third continued lifeless till sunset, when, losing all hopes of him, he was thrown away.

I wish it were possible, from this instance, to invent a method of embalming drowned persons, in such a manner that they may be recalled to life at any period, however distant; for having a very ardent desire to see

and observe the state of America a hundred years hence, I should prefer to any ordinary death, the being immersed in a cask of Madeira wine, with a few friends, till that time, to be then recalled to life by the solar warmth of my dear country! But since in all probability we live in an age too early and too near the infancy of science, to hope to see such an art brought in our time to its perfection, I must for the present content myself with the treat, which you are so kind as to promise me, of the resurrection of a fowl or a turkey cock.

I am, &c.

B. FRANKLIN.

TO WILLIAM DEANE.

A new-invented Carriage Wheel. — Explosion of Melted Iron and Glass Drops in Water. — Stove for consuming the Smoke.

London, 11 April, 1773.

DEAR SIR,

Miss Martin that was, now Mrs. Blacker, being about to return to Dublin, I cannot omit the opportunity it gives me of chatting a little with one whose conversation afforded me so much pleasure and instruction while I was there.

I know of nothing new here, worth communicating to you, unless perhaps the new art of making carriage wheels, the fellies of one piece, bent into a circle and surrounded by a hoop of iron, the whole very light and strong, there being no crossed grain in the wood, which is also a great saving of timber. The wood is first steamed in the vapor from boiling water, and then bent by a forcible machine. I have seen pieces of wood so bent of six inches wide, and three and a half thick,

into a circle of four feet diameter. These, for duration, can only be exceeded by your iron wheels. Pray, have you completed that ingenious invention?

What is become of honest Mr. Ketilby? Does he go on with his printing schemes, or has he got into some better employment?

They tell us here, that some person with you has discovered a new moving power, that may be of use in mechanical operations; that it consists in the explosion of iron tears chilled suddenly from the melting state in cold water. That explosion I have often seen in drops of glass with wonder, understanding it no more than they did in the time of Hudibras, who makes a simile of it, which I repeat because it is probably so long since you read it.

> "*Honor* is like that glassy bubble,
> That gives philosophers so much trouble;
> Whose least part cracked, the whole does fly,
> And wits are cracked to find out why."

May I ask you, if you know any thing of the application of this power, of which I have not at present the smallest conception?

I have completed my stove, in which the smoke of the coal is all turned into flame, and operates as fuel in heating the room. I have used it all this winter, and find it answer even beyond my expectations. I purpose to print a little description of its use and construction, and shall send you a copy.

I hope Billy and Jenny continue, and always will continue, as happy as when I knew them. My best wishes attend them, being as ever, with sincere esteem,

Dear Sir,

Your most obedient humble servant,

B. FRANKLIN.

TO M. DUBOURG.*

Concerning Dr. Stark.†

4 May, 1773.

——The young physician whom I mentioned is dead, and all the notes which he had left of his curious experiments are by some accident lost between our friends Sir John Pringle and Dr. Huck (Saunders); but these gentlemen, if the papers cannot be recovered, it is to be presumed, will repeat the experiments themselves.

B. FRANKLIN.

TO M. LE ROY.

Hospitals. — Effect of Cold Air on Diseases.

London, 22 June, 1773.

However glad I was of the occasion, I forbore indulging myself in the pleasure of congratulating by the first post, my dear double *confrère*, on his election into our Royal Society, because Mr. Walsh undertook to give you the information, which would make a second expense unnecessary, and I saw I should soon have this opportunity by the favor of M. Poissonnière. I rejoice in the event, as you seemed anxiously concerned about it, and as we have done ourselves honor in distinguishing and associating a merit so universally known and acknowledged.

I am pleased to hear that you are engaged in the

* Translated from M. Dubourg's edition of Franklin's Works, Vol. II. p. 312. — EDITOR.

† The works of Dr. Stark, including the experiments alluded to, have since been published.

consideration of hospitals. I wish any observations of mine could be of use to you, they should be at your service. But it is a subject I am very little acquainted with. I can only say, that, if a free and copious respiration is of use in diseases, that seems, from the experiments I mentioned to M. Dubourg, to be best obtained by light covering and fresh air continually changing; the moisture on the skin, when the body is warmly covered, being a deception, and the effect, not of greater transpiration, but of the saturation of the air included under and in the bedclothes, which therefore can absorb no more, and so leaves it on the surface of the body. From these experiments I am convinced of what I indeed before suspected, that the opinion of perspiration being checked by cold is an error, as well as that of rheum being occasioned by cold. But as this is heresy here, and perhaps may be so with you, I only whisper it and expect you will keep my secret. Our physicians have began to discover that fresh air is good for people in the small-pox, and other fevers. I hope in time they will find out, that it does no harm to people in health.

We have nothing new here in the philosophic way. I shall like to hear how M. Lavoisier's doctrine supports itself, as I suppose it will be controverted.

With the greatest esteem, I am ever, Dear Sir,
Yours most affectionately,
B. FRANKLIN.

P. S. Enclosed I send you some pamphlets relative to our American affairs for your amusement. Sir John Pringle bids me present his compliments. He interested himself much in the election.

TO M. DUBOURG.

Inquiries on the Causes of Taking Cold.

London 29 June, 1773.

DEAR FRIEND,

I have not time now to write what I intend upon the cause of colds, or rheums; and my opinions on that head are so singular here, that I am almost afraid to hazard them abroad. In the mean time, be so kind as to tell me at your leisure whether in France you have a general belief, that moist air, and cold air, and damp shirts or sheets, and wet floors, and beds that have not been lately used, and clothes that have not been lately worn, and going out of a warm room into the air, and leaving off a long-worn waistcoat, and wearing leaky shoes, and sitting near an open door or window, or in a coach with both glasses down, are all or any of them capable of giving the distemper we call *a cold*, and you *a rheum*, or *catarrh*? Or are these merely *English* ideas?

I am ever, with the greatest esteem and respect,

Dear Sir, yours, &c.

B. FRANKLIN.

Preparatory Notes and Hints for writing a Paper concerning what is called Catching Cold.

Definition of a Cold.

IT is a siziness and thickness of the blood, whereby the smaller vessels are obstructed, and the perspirable matter retained, which being retained offends both by its quantity and quality; by quantity, as it outfills the vessels, and by its quality, as part of it is acrid, and being retained, produces coughs and sneezing by irritation.

How this Sizziness is produced.

1. By being long exposed in a cold air, without exercise; cold thickens glue.

2. By a diminished perspiration, either first from breathing and living in moist air, or, second, from the clogging of the pores by clammy sweat dried on and fastening down the scales of the skin; or, thirdly, by cold constringing the pores partially or totally, sleeping or waking; or, fourthly, by having eat food of too gross particles for free perspiration, as oysters, pork, ducks, &c. People are found frequently costive after much bathing.

3. By repletion, as when more is thrown into the habit by eating and drinking than common perspiration is capable of discharging in due time; whence the vessels are distended beyond their spring, and the quantity of contained fluid, that should be briskly moved to preserve or acquire a due thinness, is too weighty for their force, whence a slow motion,— thence viscidity. This repletion is increased by a constipation of the belly happening at the same time. In an approaching cold, more water is made than usual.

4. By cooling suddenly in the air after exercise. Exercise quickening the circulation, produces more perspirable matter in a given time, than is produced in rest. And though more is likewise usually discharged during exercise, yet on sudden quitting of exercise, and standing in the air, the circulation and production of perspirable matter still continuing some time, the over quantity is retained. It is safer not to go into water too cold.

5. By particular effluvia in the air, from some unknown cause. General colds throughout a country. By being in a coach close, or small room with a person having a cold.

6. By relaxation of the solids, from a warm and moist air, so that they are too weak to give due motion to the fluids.

Of partial colds affecting parts only of the body.

Causes of feverishness attending colds.

Ill consequences often attending colds, as pleurisies, consumptions, &c. Some never taking cold, some frequently; cause of the difference.

Present remedies for a cold should be warming, diluting, bracing.

Means of preventing cold; temperance, choice of meats and drinks, warm rooms, and lodging, and clothing in winter; dry air, care to keep the belly open, and frequent discharge of water; warm bathing to cleanse the skin; rubbing after sweat, especially in the spring.

Difficulties that first put me on thinking on this subject. People get cold by less, and not by more, viz.

By putting on a damp shirt on a dry body,—Yes.

By putting on a dry shirt on a wet body, though this wets the shirt ten times more,—No.

By sitting in a room, where the floor has been newly washed,—Yes.

By going into a river, and staying there an hour (no sheets so wet),—No.

By wetting the feet only,—Yes.

By wetting all the clothes through to the body, and wearing them a whole day,—No.

By sitting in a room against a crevice,—Yes.

By sitting as long in the open air,—No.

Few of these effects take place, if the vessels are kept empty.

Reapers in Pennsylvania;—

Drinking cold water when they are hot.

If it makes them sweat, they are safe,

If not, they fall ill, and some die.

People hot, should drink by spoonfuls; the reason.

Taking cold. The disorder only called so in English, and in no other language.

American Indians, in the woods, and the whites in imitation of them, lie with their feet to the fire in frosty nights, and *take no cold* while they can keep their feet warm.

Feet and hands apt to be cold in that disorder, and why. Is it the siziness, or the greater evaporation?

Hottentots grease themselves,—occasions other evacuations more plentiful. Greasing keeps the body warm. Bad to hold the water too long. Parts colder when first unclothed than afterwards, why?

It was a disgrace among the ancient Persians to cough or spit.

Probably as it argued intemperance.

Vessels when too full, leak. Quicksilver through leather. Thin fluid leaked evaporates. Corners of eyes, &c. Sizy will not all evaporate. What is left corrupts. Hence consumptions. Hectic fevers, from absorption of putrid pus. It ferments the blood like yeast.

People seldom get cold at sea, though they sleep in wet clothes. Constant exercise, moderate living. Bad cooks. Yet air is very moist. Wet floors. Sea surrounding, &c.

Exercise cures a cold. Bishop Williams riding several times from London, or Exeter, to Salisbury.

Bark good for a cold, taken early.

Particular parts more accustomed to discharge the irritating perspirable matter, as under the arms in some, feet in others, &c.

Experiment of two razors.

Every pain or disorder now ascribed to a cold.

It is the covering excuse of all intemperance.

Numbers of people in a close room, and exercising there, fill the air with putrid particles.

People killed by House of Commons, breathing the air through holes in ceiling.

Think they get cold by coming *out* of such hot rooms; they get them by being *in*.

Those who live in hotter rooms (stoves) get no colds;

Germans and all the northern people.

Alderman and turtle.

People remark, they were very well before a cold, and eat hearty. Wonder how they catched it.

Signs of Temperance.

Mouth not clammy after sleep.
Saliva thin and watery.
Eyelids not stuck together with hard glue.
Voice clear.
No phlegm to raise.

Advice for mode of general temperance without appearing too singular.

Supper not bad after preparatory light dinner.

May be rectified by slight breakfast next morning.

He must be too full that one excess will much disorder.

Time of great meal mended of late.

One hour variation of compass in twenty years.

After dinner not fit for business.

People from the country get cold when they come to London, and why? Full living with moist air. London air generally moist, why? Much putrid air in London. Silver, &c.

Cooks and doctors should change maxims.

Common sense more common among the common Scotch.

Those who do not compare, cannot conceive the difference between themselves and themselves in full or spare living.

Wet newspapers, why give colds.

Old libraries, and damp old books.

Putrid animal matter in paper size.

Courts should not sit after dinner.

Juries fast, a good institution.

Chess,—Impatience of deliberation because more difficult. Writing, &c.

Most follies arise from full feeding. Reasons *pro* and *con* not all present.

Temperate nations wisest.

Dining entertainments bad.

Remains of barbarism,—expensive.

Full feeding of children stupefies.

Fasting strengthens reason rather than subdues passion.

People often do not get cold when they think they do, and do when they think they do not.

Causes of colds are primary and secondary.

Colds are of different kinds, putrid and plethoric.

Scarce any air abroad so unwholesome as air in a close room often breathed.

Warm air dissolves more moisture than cold.

In hot countries men wrap themselves in wet sheets to sleep.

A general service to redeem people from the slavish fear of getting cold, by showing them where the danger is not, and that where it is, it is in their power to avoid it.

Surfeit, an expression formerly used, now laid aside.

Costiveness occasioning colds, how to be prevented.

Colds formerly called rheums and catarrhs.

Particular foods said to engender rheums.

Query. Is Mr. Wood more or less subject to catch cold since he betook himself to his low diet?

Answer (by Mr. Wood). He now finds himself *much more* healthy, and *much less* liable to catch cold. What few colds he now catches are so very slight, that he is not sensible of them, but from the urine, which is then not so clear.

I caused the above question to be asked Mr. Wood, and obtained the answer. It is the Mr. Wood who lives upon a pound of flour in a pudding.

B. FRANKLIN.

Dampier, speaking of the customs of the people at Mindanoo, (p. 330,) says; "You see abundance of people in the river from morning to night washing their bodies or clothes; they strip and stand naked till they have done; then put them on and march out again."

Dr. Gregory says; "All that class of diseases which arise from catching cold, is found only among the civilized part of mankind. An old Roman or an Indian, in the pursuit of war or hunting, would plunge into a river whilst in a profuse sweat, without fear, and without danger. The greater care we take to prevent catching cold, by the various contrivances of modern luxury, the more we become subject to it. We can guard against cold only by rendering ourselves superior to its influence. There is a striking instance of this in the vigorous constitutions of children who go thinly clad in all seasons and weathers."

The coats of the vessels are a kind of network, which contains the fluids only when not so pressed as to enlarge the pores of the net, or when the fluids are not so pressed as to break the cohesion of the globules or particles, so as to make them small enough to come

through. When the vessels are full, occasioned by a course of full living, they labor in carrying on the circulation; their spring or power of contraction and compressing the fluids they contain, being overstrained, is weakened, the circulation proceeds more slowly, the fluids thicken and become more gluey, both for want of due churning and because less heat is produced in the body. Such a body requires more aid of clothing and fire to preserve its warmth.

If a person in that state of body walks a mile or two, or uses any other exercise that warms him, the fluids are rarefied by the heat, distend the vessels still more, and the thinner parts of the fluids in tender places force out through the pores of the vessels in form of a gluey water, viz. at the eyes, within the nose, and within the lungs. This in moderate exercise.

If the exercise is increased it comes through every pore in the skin, and is called sweat.

The more volatile parts of this extravasated fluid evaporate, and fly off in the air; the gluey part remains, thickens and hardens more or less, as it becomes more or less dry; in the nose and on the lungs, where air is continually coming and going, it soon becomes a mucus, but can hardly grow drier because surrounded with moist parts and supplied with more moisture. What oozes out of the corner of the eye when shut, as in sleep, hardens into what is called a kind of gum, being in fact dry glue.

This in a morning almost sticks the eyelids together.

With such mucous matter the nose is sometimes almost stopped, and must be cleared by strong blowing.

In the windpipe and on the lungs it gathers and is impacted, so as sometimes to induce a continual coughing and hawking to discharge it.

If not easily discharged, but remaining long adhering to the lungs, it corrupts and inflames the parts it is in contact with; even behind the ears and between the parts of the body so constantly in contact, that the perspirable matter, sweat, &c. cannot easily escape from between them; the skin is inflamed by it, and a partial putrefaction begins to take place, they corrupt and ulcerate. The vessels being thus wounded, discharge greater and continual quantities. Hence consumption.

Part of the corrupted matter, absorbed again by the vessels and mixed with the blood, occasions hectic fevers.

When the body has sweated, not from a dissolution of fluids, but from the force above mentioned, as the sweat dries off, some clammy substance remains in the pores, which closes many of them, wholly or in part. The subsequent perspiration is hereby lessened.

The perspirable matter consists of parts approaching to putrefaction, and therefore destined by nature to be thrown off, that living bodies might not putrefy, which otherwise, from their warmth and moisture, they would be apt to do.

These corrupting particles, if continually thrown off, the remainder of the body continues uncorrupt, or approaches no nearer to a state of putrefaction. Just as in boiling water, no greater degree of heat than the boiling heat can be acquired, because the particles that grow hotter, as fast as they become so, fly off in vapor. But if the vapor could be retained, water might be made much hotter, perhaps red-hot, as oil may, which is not so subject to evaporation. So if the perspirable matter is retained it remixes with blood, and produces first, a slight putrid fever, attending always what we call a cold, and when retained in a great degree, more mischievous putrid diseases.

In hot countries, exercise of body with the heat of the climate create much of this putrid perspirable matter, which ought to be discharged. A check is in those countries very pernicious; putrid malignant violent fevers, and speedy death, the consequence.

Its discharge is also checked another way besides that of closing the pores, viz. by being in an air already full of it, as in close rooms containing great numbers of people, playhouses, ballrooms, &c.

For air containing a quantity of any kind of vapor, becomes thereby less capable of imbibing more of that vapor, and finally will take no more of it.

If the air will not take it off from the body, it must remain in the body; and the perspiration is as effectually stopped, and the perspirable matter as certainly retained, as if the pores were all stopped.

A lock of wet wool contained in a nutmeg-grater, may dry, parting with its moisture through the holes of the grater. But if you stop all those holes with wax it will never dry. Nor, if exposed to the open air, will it dry when the air is as moist as itself. On the contrary, if already dry, and exposed to moist air, it would acquire moisture.

Thus people in rooms heated by a multitude of people, find their own bodies heated; thence the quantity of perspirable matter is increased that should be discharged, but the air, not being changed, grows so full of the same matter, that it will receive no more. So the body must retain it. The consequence is, that next day, perhaps sooner, a slight putrid fever comes on, with all the marks of what we call a cold, and the disorder is supposed to be got by coming out of a warm room, whereas it was really taken while in that room.

Putrid ferments beget their like. — Small-pox. —

Wet rotten paper, containing corrupt glue. The cold fever communicable by the breath to others, &c.

Urine retained, occasions sneezing, &c.

Coughing and spitting continually, marks of intemperance.

People eat much more than is necessary.

Proportionable nourishment and strength is not drawn from great eating.

The succeeding meals force the preceding through half-undigested.

Small meals continue longer in the body, and are more thoroughly digested.

The vessels being roomy can bear and receive without hurt, an accidental excess.

They can concrete more easily.

There is less quantity of corrupting particles produced.

Putrid fish very bad.

Black Hole in the Indies.

TO SAMUEL COOPER.*

Stoves for Public Buildings.

London, 7 July, 1773.

I congratulate you on the finishing of your new meetinghouse. I have considered, as well as I can, without being on the spot, the intention of warming it by some machine in the cold damp seasons. It must be a matter of difficulty to warm sensibly all the air in so large and so lofty a room, especially if the fire is

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* Dr. Samuel Cooper, an eminent clergyman of Boston, with whom Dr. Franklin held a correspondence for many years. — EDITOR

not kept up in it constantly on the week days as well as Sundays. For, though the machine is very large and made very hot, yet the space of air and quantity of wall to be warmed is so great, that it must be long before any considerable effect will be produced. Then it will descend by the walls and windows, which being very cold by the preceding week's absence of fire, will cool that descending air so much in so long a descent, that it will fall very heavily and uncomfortably upon the heads of all that happened to sit under it, and will proceed in cold currents along the floor to the warming machine wherever it is situated. This must continue till the walls are warmed, for which I think one day is by no means sufficient, and that therefore a fire kindled in the morning of the Sabbath will afford no comfort to the congregation that day, except to a few that sit near it, and some inconvenience to the rest from the currents above mentioned.

If, however, your people, as they are rich, can afford it, and may be willing to indulge themselves, should choose to keep up a constant fire in the winter months, you may have from this country a machine for the purpose, cast from the same patterns with those now used at the Bank, or that in Lincoln's Inn Hall, which are placed in the middle of the respective rooms. The smoke of these descends, and passing under ground, rises in some chimney at a distance. Yours must be a chimney built, I suppose, without the house; and, as it ought to draw well to prevent your being troubled with smoke (as they often are at the Bank), it should be on the south side; but this I fear would disfigure your front. That at Lincoln's Inn Hall draws better. They are in the form of temples, cast in iron, with columns, cornices, and every member of elegant architecture.

And I mention casting them from the same patterns or moulds, because, those being already made, a great deal of work and expense will thereby be saved. But if you can cast them in New England, a large vase, or an antique altar, which are more simple forms, may answer the purpose as well, and be more easily executed. Yet after all, when I consider the little effect I have observed from these machines in those great rooms, the complaints of people who have tried Buzaglo's stoves in halls, and how far your meeting-house must exceed them in all its dimensions, I apprehend, that after a great deal of expense, and a good deal of dust on the seats and in the pews, which they constantly occasion, you will not find your expectations answered. And, persuaded as I am, from philosophic considerations, that *no one ever* catches the disorder we call a cold from cold air, and therefore never at meeting, I should think it rather advisable to those who cannot well bear it, to guard against the short inconvenience of cold feet (which only takes place towards the end of the service), by basses or bearskin cases to put the legs in, or by small stoves with a few coals under foot, *more majorum.*

B. FRANKLIN.

TO BENJAMIN RUSH.

Colds produced by Contagion. — Various Causes of Colds.

London, 14 July, 1773.

DEAR SIR,

I received your favor of May 1st, with the pamphlet, for which I am obliged to you. It is well written. I hope that in time the endeavours of the friends to

liberty and humanity will get the better of a practice, that has so long disgraced our nation and religion.

A few days after I received your packet for M. Dubourg, I had an opportunity of forwarding it to him per M. Poissonnière, physician of Paris, who kindly undertook to deliver it. M. Dubourg has been translating my book* into French. It is nearly printed, and he tells me he purposes a copy for you.

I shall communicate your judicious remark, relating to the septic quality of the air transpired by patients in putrid diseases, to my friend Dr. Priestley. I hope that after having discovered the benefit of fresh and cool air applied to the sick, people will begin to suspect that possibly it may do no harm to the well. I have not seen Dr. Cullen's book, but am glad to hear that he speaks of catarrhs or colds by contagion. I have long been satisfied from observation, that besides the general colds now termed *influenzas*, (which may possibly spread by contagion, as well as by a particular quality of the air), people often catch cold from one another when shut up together in close rooms, coaches, &c., and when sitting near and conversing so as to breathe in each other's transpiration; the disorder being in a certain state. I think, too, that it is the frouzy, corrupt air from animal substances, and the perspired matter from our bodies, which being long confined in beds not lately used, and clothes not lately worn, and books long shut up in close rooms, obtains that kind of putridity, which occasions the colds observed upon sleeping in, wearing, and turning over such bedclothes, or books, and not their coldness or dampness. From these causes, but more from too full living, with too little exercise, proceed in my opinion most of the dis-

* See remarks on this translation, Vol. V. p. 180.

orders, which for about one hundred and fifty years past the English have called *colds.*

As to Dr. Cullen's cold or catarrh *a frigore,* I question whether such an one ever existed. Travelling in our severe winters, I have suffered cold sometimes to an extremity only short of freezing, but this did not make me *catch cold.* And, for moisture, I have been in the river every evening two or three hours for a fortnight together, when one would suppose I might imbibe enough of it to *take cold* if humidity could give it; but no such effect ever followed. Boys never get cold by swimming. Nor are people at sea, or who live at Bermudas, or St. Helena, small islands, where the air must be ever moist from the dashing and breaking of waves against their rocks on all sides, more subject to colds than those who inhabit part of a continent where the air is driest. Dampness may indeed assist in producing putridity and those miasmata which infect us with the disorder we call a cold; but of itself can never by a little addition of moisture hurt a body filled with watery fluids from head to foot.

With great esteem, and sincere wishes for your welfare, I am, Sir,

Your most obedient humble servant,

B. FRANKLIN.

TO THOMAS PERCIVAL.*

Unwholesomeness of Manufacturing Establishments. — Moist Air not unhealthy.

West Wycombe, the Seat of Lord Le Despencer, 25 September, 1773.

DEAR SIR,

I have received here your favor of the 18th, enclosing your very valuable paper of the enumeration of Manchester. Such inquiries may be as useful as they are curious, and if once made general, would greatly assist in the prudent government of a state.†

The difference of deaths between one in twenty-eight at Manchester, and one in one hundred and twenty at Morton is surprising. It seems to show the unwholesomeness of the manufacturing life; owing perhaps to the confinement in small, close rooms, or in larger with numbers, or to poverty and want of necessaries, or to drinking, or to all of them. Farmers who manufacture in their own families what they have occasion for and no more, are perhaps the happiest people and the healthiest.

It is a curious remark, that moist seasons are the healthiest. The gentry of England are remarkably afraid of moisture, and of air. But seamen, who live in perpetually moist air, are always healthy, if they have good provisions. The inhabitants of Bermuda, St. Helena, and other islands far from continents, surrounded with rocks, against which the waves continually dashing, fill the air with spray and vapor, and where

* A learned and eminent physician of Manchester, in England, and author of several valuable publications on medical and philosophical subjects. — EDITOR.

† An extract from this letter, concerning the provision made in China against famine, is printed, under the head of "Political Economy," in Volume II. p. 381. — EDITOR.

no wind can arrive, that does not pass over much sea, and of course bring much moisture, these people are remarkably healthy. And I have long thought, that mere moist air has no ill effect on the constitution; though air impregnated with vapors from putrid marshes is found pernicious, not from the moisture, but the putridity. It seems strange that a man, whose body is composed in great part of moist fluids, whose blood and juices are so watery, who can swallow quantities of water and small beer daily without inconvenience, should fancy that a little more or less moisture in the air should be of such importance. But we abound in absurdity and inconsistency.

Thus, though it is generally allowed that *taking the air* is a good thing, yet what caution against air! What stopping of crevices! What wrapping up in warm clothes! What stuffing of doors and windows! even in the midst of summer. Many London families go out once a day to take the air; three or four persons in a coach, one perhaps sick; these go three or four miles, or as many turns in Hyde Park, with the glasses both up close, all breathing over and over again the same air they brought out of town with them in the coach, with the least change possible, and rendered worse and worse every moment. And this they call *taking the air*. From many years' observations on myself and others, I am persuaded we are on a wrong scent in supposing moist or cold air the causes of that disorder we call *a cold*. Some unknown quality in the air may perhaps produce colds, as in the *influenza;* but generally I apprehend they are the effect of too full living in proportion to our exercise.

Excuse, if you can, my intruding into your province, and believe me ever with sincere esteem, dear Sir,

Your most obedient humble servant,

B. FRANKLIN.

FROM JOSEPH PRIESTLEY TO B. FRANKLIN.

New Experiments on Air.

Calne, 26 September, 1773.

DEAR SIR,

With this I return you Mr. Winthrop's letter* according to your desire, thanking you for your endeavours to serve me in America, though I find, as I was apprehensive, that the scheme would not answer. Please to return my thanks to the Professor for his candid and judicious remarks on my *History of Optics*, which will be much improved by them, if it should come to a second edition.

Dr. Price will have informed you, that I have resumed my experiments on *air*, and with a good prospect of success. Since that, I have been much more successful; but in a letter I can only confine myself to the heads of things.

The most important of the observations I have lately made is of an *alkaline* corresponding to my *acid air*, of which an account is given in what I have already printed. This I get by treating a *volatile alkali* in the same manner in which I before treated the *spirit of salt*. As soon as the liquor begins to boil, the vapor arises, and being received in a vessel filled with quicksilver continues in the form of *air* well condensed by cold.

I imagined that a mixture of this alkaline with acid air would make a neutral, and perhaps a common air; but, instead of that, they make a beautiful white salt, of a very curious nature. It immediately deliquesces, and even wholly disappears, upon being exposed to the open air; and if it be in a dry and deep vessel, where moisture cannot easily come at it, it wholly evaporates in dense white clouds, occasioning a very strong smell.

* See above, p. 375.

This alkaline vapor, like the acid, is quickly imbibed by water, which thereby becomes spirit of *sal volatile.*

Nitrous air makes this alkaline vapor turbid, and perhaps generates a different salt. But I have not yet made a tenth part of the experiments, that I propose to do with this new kind of air.

I have just found that spirit of wine yields air also, which is probably pure phlogiston, but I have not yet made one experiment with it.

If volatile alkali, liquid or solid, be exposed to nitrous air during its effervescences with common air, the vessel is presently filled with beautiful white clouds, and the salt is tinged blue. This explains the constitution of nitrous air, but I have no time for reasoning.

This experiment appears to great advantage when a vessel, no matter how large, containing the smallest portion of volatile alkali, fluid or solid, is opened in a quantity of nitrous air, for the whole is filled almost instantly with dense white clouds.

Report says, that you are about to leave us, at least for a time. If this be true, I shall be very sorry, as it will deprive me of one of the greatest satisfactions that used to make my annual visits to London agreeable. If you should leave England before winter, I should think myself very happy in an opportunity of seeing you before your departure. As I cannot conveniently come to London, I should be particularly happy in seeing you and Sir John Pringle at my new situation, and I flatter myself that I could amuse you with some of my new experiments. If you can oblige me so far, give me a line to acquaint me with your intention, that I may be sure to be at home when you come.

I am, dear Sir, yours sincerely,

JOSEPH PRIESTLEY.

TO JOHN INGENHOUSZ.

Torpedo. — Magnetic Needle. — Nairne's Electrical Machine. — Magazines at Purfleet.

London, 30 September, 1773.

MY DEAR FRIEND,

I rejoiced as much as any friend could do at the news we received here from time to time of your success in your profession, and of the safe recovery of your illustrious patients of that most amiable family. But it grieved us all, at the same time, to hear that you did not yourself enjoy health in that country. Surely their known goodness will graciously give you leave of absence, if you have but the courage to request it, and permit you to come and reside in England, which always agreed well with your constitution. All your friends here will be made happy by such an event.*

I had purposed to return to America this last summer, but some events in our Colony affairs induced me to stay here another winter. Some time in May or June next I believe I shall leave England. May I hope to see you here once more?

I shall be glad to see the work of the Abbé Fontana on that disease of wheat. As yet I have not heard that it is come to England.

Sir William Hamilton writes from Naples, that after

* Dr. Ingenhousz was now residing at Vienna, whither he had gone to inoculate for the smallpox the Archduchess Theresa Elizabeth, the only daughter of the Emperor, and the Archdukes Ferdinand and Maximilian, the Emperor's brothers. He remained in that city several years. He was in England during a large part of the year 1779, when he published his work, entitled *Experiments on Vegetables*, &c. In the title-page of that work, he styles himself, "Counsellor of the Court and Body Physician to their Imperial Majesties." — EDITOR.

many experiments he has not been able to perceive any certain signs of electricity in the torpedo. It is perhaps best that there should be two opinions on this subject, for that may occasion a more thorough examination of it, and finally make us better acquainted with it.

It is not difficult to construct a needle, so as to keep pointing to the meridian of any one place, whatever may be the variation in that place. But to point always to the meridian, wherever the needle may be removed, is, I apprehend, not possible.

Mr. Nairne has, as you have heard, finished a very fine electric machine. I have seen sparks from the prime conductor thirteen inches in length. He has added a large battery, and produces a discharge from it sufficiently strong to blast growing vegetables, as lightning is supposed to do. From a greater force used, perhaps some more discoveries may be made. I am much pleased with the account you give me of your new machine of white velvet rubbed upon hare-skin.

Last year the Board of Ordnance applied to the Royal Society here for their opinion of the propriety of erecting conductors to secure the powder magazines at Purfleet. The Society appointed a committee to view the magazines, and report their advice. The members appointed were Messrs. Cavendish, Watson, Delaval, Robertson, Wilson, and myself. We accordingly, after viewing them, drew up a Report, recommending conductors to each, elevated ten feet above the roof, and pointed at the ends. Mr. Delaval did not attend; all the rest agreed in the Report, only Mr. Wilson objected to pointing the rods, asserting that blunt ends or knobs would be better. The work, however, was finished according to our direction. He

was displeased, that his opinion was not followed, and has written a pamphlet against points. I have not answered it, being averse to disputes. But in a new translation and edition of my book printed lately at Paris, in two volumes, quarto, you will see some new experiments of mine, with the reasonings upon them, which satisfied the committee. They are not yet printed in English, but will be in a new edition now printing at Oxford, and perhaps they will be in the next Transactions.*

It has been a fashion to decry Hawkesworth's book; but it does not deserve the treatment it has met with. It acquaints us with new people having new customs, and teaches us a good deal of new knowledge.

Captain Phips has returned, not having been able to approach the Pole nearer than eighty-one degrees, the ice preventing.

M. Tremont, an ingenious young Italian, who was lately here, gave me a little spyglass of his making, upon Père Boscovich's principles, the ocular lens being a composition of different glasses instead of the objective. It is indeed a very good one.

Sir John Pringle is returned from Scotland, better in health than heretofore. He always speaks of you

* See these experiments in Vol. V. p. 435. A copy of M. Dubourg's French translation of Dr. Franklin's writings, above alluded to, was sent by him to Harvard College. The following complimentary vote of the Corporation was returned.

"At a meeting of the Corporation of Harvard College, May 31st, 1774.

"Dr. Franklin having presented to our Library a French Translation of his Philosophical Writings in two volumes quarto, a copy which we receive with particular pleasure, as it is a testimony of the sense foreigners have of the merit of these writings, which must do honor to the country that gave him birth, as well as to every literary Society he is related to; Voted, that the thanks of this Board be given to Dr. Franklin for this valuable present, and that Dr. Cooper do transmit him a copy of this vote. NATHANIEL APPLETON,

"*Senior Fellow of the Corporation.*"

with respect and affection, as does Dr. Huck and all that knew you.

I am ever, with the sincerest esteem, dear Sir,

Your faithful and most obedient servant,

B. FRANKLIN.

FROM JOSEPH PRIESTLEY TO B. FRANKLIN.

Experiments on Alkaline Air.

Calne, 14 October, 1773.

DEAR SIR,

I hope you received my letter, in which I gave you some account of my discovery of an *alkaline air*, though I have not had the pleasure of hearing from you since. I am still busy in examining its properties and affinities, some of which are curious enough. The most remarkable observation I have made is, of the manner in which it affects *alum* put into it. This substance absorbs it very fast, and then comes out perfectly white, and altogether unlike what it was; but I have not yet examined it any further.

I have also found, that this *alkaline air* is slightly *inflammable*. This is not observed without attention, for a candle dipped into it goes out several times before there is much appearance of its inflammability. This, however, agrees with the opinion of chemists, that volatile alkali contains phlogiston.

As you have not written to me, I hope you are planning an excursion to Calne with Sir John Pringle, or some of our friends. This would make me very happy. I am, with great respect, &c.

J. PRIESTLEY.*

* Six weeks after the date of the above letter, that is, on the 30th of November, 1773, Dr. Priestley received from the Royal Society the

FROM ERASMUS DARWIN TO B. FRANKLIN.*

Communicating a Philosophical Paper.

Lichfield, 24 January, 1774.

DEAR SIR,

I have enclosed a medico-philosophical paper, which I should take it as a favor if you will communicate to

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Copley Medal, in honor of his discoveries of various properties of air. For the Discourse of the President of the Royal Society on the occasion, see SIR JOHN PRINGLE'S *Six Discourses*, p. 1.

Before the Council of the Society had decided on bestowing the medal, Dr. Franklin was requested to examine Dr. Priestley's book and report his opinion. To a nobleman, a member of the Council, (whose name is not written on the draft of the letter that remains,) he replied as follows.

"My Lord; In obedience to your Lordship's commands, I have looked over that part of Dr. Priestley's work, that contains an account of the experiments made by him. I find there is a great number of them, mostly quite new, and some I think very curious and important, well deserving for that reason, and for the great pains and expense he has been at in making them, the honor of the Society's medal. I am, however, so engaged at present, that I could not possibly prepare any thing of the kind, fit to be laid before the Council on Thursday, and hope your Lordship will be so good as to excuse me. I do not see that his account of them can well be abridged; and, as the book has been long published, and probably is in the hands of all those of the Society, who, from their acquaintance with the subject, are the best judges of the merit of such experiments, I apprehend that any other account is hardly necessary. With the greatest respect," &c. — EDITOR.

* An acquaintance of long standing, and a correspondence on philosophical subjects, seem to have subsisted between Dr. Franklin and Dr. Darwin, but none of Franklin's letters to Darwin have come within my researches. The only record of their intercourse, contained in Miss Seward's "Memoirs of Dr. Darwin," is the following anecdote. "When he wrote to Dr. Franklin, complimenting him on having united philosophy to modern science, he directed his letter merely thus, '*Dr. Franklin, America*,' and said he felt inclined to make a still more flattering superscription; '*Dr. Franklin, the World.*' His letter reached the sage, who first disarmed the lightning of its fatal power, for the answer to it arrived, and was shown in the Darwinian circles; in which had been questioned the likelihood of Dr. Franklin ever receiving a letter of such general superscription, as the whole western empire." SEWARD'S *Memoirs of Dr. Darwin*, p. 152. — EDITOR.

the Royal Society, if you think it worthy a place in their volume; otherwise, I must desire you to return it to the writer. I have another very curious paper containing experiments on the colors seen in the closed eye, after having gazed some time on luminous objects, which is not quite transcribed, but which I will also send to you, if you think it is likely to be acceptable to the Society at this time, but will otherwise let it lie by me another year.

I hope you continue to enjoy your health, and that I shall some time again have the pleasure of seeing you at Staffordshire. I am, dear Sir,

Your affectionate friend,

ERASMUS DARWIN.

TO THE MARQUIS DE CONDORCET.

Answers to Questions on Philosophical Subjects.

London, 20 March, 1774.

SIR,

I am ashamed that my late continued *embarras* in public affairs should have so long prevented my answering the letter you honored me with, of the 2d of December last.

I transmitted your queries to our Society at Philadelphia, where they will be well considered, and full answers will be sent to you. On my return thither, which I am now preparing for, I shall take care, if not done, to urge the doing it as soon as possible.

In the mean time, I can inform you, as to question first, that, though there is in Pennsylvania abundance of limestone and marble, no flint has yet been found there by the English; yet it is supposed, that flint is to be met with in some part of the country, since heads

of arrows made of it by the ancient inhabitants are sometimes found in ploughing the fields. Thus, small sea shells are found intermixed with the substance of rock-stone in some of our highest mountains, and such I think as are not now to be met with on our coasts. Several skeletons, supposed by their tusks to be of elephants, have been found near the Ohio, an account of which may be found in the English Philosophical Transactions.

As to question second, observations have been made in America of the variation of the needle, and, as well as I can remember, it is found to differ a degree in about twenty years.

As to question third; the height of the barometer, by many years' observation, is said to vary between 28·59 and 30·78. The conjectures from these changes are still uncertain.

As to question fourth; the negroes, who are free, live among the white people, but are generally improvident and poor. I think they are not deficient in natural understanding, but they have not the advantages of education. They make good musicians.

As to question fifth; I do not know that any marks of volcanoes, any lava, or pomice-stone, have been met with in North America. Pit-coal is found in many places, and very good, but little used, there being plenty of wood.

These answers are very short. I hope to procure you such as shall be more full and satisfactory.

With great respect, I have the honor to be, Sir, &c.

B. FRANKLIN.

TO JOHN BAPTIST BECCARIA.

English Translation of Beccaria's Book. — Experiment to show that Electricity does not pass through a perfect Vacuum.

London, 20 March, 1774.

REVEREND AND DEAR SIR,

I have received several of your favors lately, relating to the edition of your book in English, which I have put into the hands of the translator, who will observe your directions. The work is now in the press, and goes on pretty fast. I am much obliged by your kind assistance in procuring the impressions from the plates. They are not yet arrived here; but the money, which I find by a note from you to Dr. Priestley amounts to one hundred and forty-three livres of Piedmont, will be paid by the bookseller, Mr. Nourse, in my absence, to any person you may order to receive it.

Mr. Walsh, the same ingenious member of our Society who went to France to make experiments on the Torpedo, has lately hit on a new discovery in electricity, which surprises us a little. You know that finding air, made rarer by the pump or by heat, gave less obstruction to the passage of electricity, than when in its denser state, we were apt to think a perfect vacuum would give it no resistance at all. But he, having by boiling the mercury made a perfect vacuum in a long bent Torricellian tube, has found that vacuum absolutely to resist the passage of the electric fluid during two or three days, or till some quantity of air, the smallest imaginable, is admitted into it. This, if verified by future experiments, may afford some new light to the doctrine ——*

* The remainder of the letter is lost. — EDITOR.

TO JOSEPH PRIESTLEY.*

Effect of Vegetation on Noxious Air.

—— That the vegetable creation should restore the air which is spoiled by the animal part of it, looks like a rational system, and seems to be of a piece with the rest. Thus fire purifies water all the world over. It purifies it by distillation, when it raises it in vapors, and lets it fall in rain; and farther still by filtration, when, keeping it fluid, it suffers that rain to percolate the earth. We knew before, that putrid animal substances were converted into sweet vegetables, when mixed with the earth, and applied as manure; and now it seems, that the same putrid substances, mixed with the air, have a similar effect. The strong thriving state of your mint, in putrid air, seems to show, that the air is mended by taking something from it, and not by adding to it. I hope this will give some check to the rage of destroying trees that grow near houses, which has accompanied our late improvements in gardening, from an opinion of their being unwholesome. I am certain, from long observation, that there is nothing unhealthy in the air of woods; for we Americans have everywhere our country habitations in the midst of woods, and no people on earth enjoy better health, or are more prolific. ——

B. FRANKLIN.

* This extract is taken from Priestley's *Experiments on Air*, (Vol. I. p. 94,) 3d Edition. The author introduces it with the following remark. "Dr. Franklin, who, as I have already observed, saw some of my plants in a very flourishing state, in noxious air, was pleased to express very great satisfaction with the result of the experiments. In answer to the letter in which I informed him of it, he says," &c. — EDITOR.

TO JOSEPH PRIESTLEY.*

On the Inflammability of the Surface of certain Rivers in America.

Craven Street, 10 April, 1774.

DEAR SIR,

In compliance with your request, I have endeavoured to recollect the circumstances of the American experiments I formerly mentioned to you, of raising a flame on the surface of some waters there.

When I passed through New Jersey in 1764, I heard it several times mentioned, that, by applying a lighted candle near the surface of some of their rivers, a sudden flame would catch and spread on the water, continuing to burn for near half a minute. But the accounts I received were so imperfect, that I could form no guess at the cause of such an effect, and rather doubted the truth of it. I had no opportunity of seeing the experiment; but, calling to see a friend who happened to be just returning home from making it himself, I learned from him the manner of it; which was to choose a shallow place, where the bottom could be reached by a walking-stick, and was muddy; the mud was first to be stirred with the stick, and, when a number of small bubbles began to arise from it, the candle was applied. The flame was so sudden and so strong, that it catched his ruffle and spoiled it, as I saw. New Jersey having many pine-trees in many parts of it, I then imagined that something like a volatile oil of turpentine might be mixed with the waters from a pine-swamp, but this supposition did not quite satisfy me.

* See Priestley's *Experiments on Air*, (Vol. I. p. 321,) 3d Edition.—EDITOR.

I mentioned the fact to some philosophical friends on my return to England, but it was not much attended to. I suppose I was thought a little too credulous.

In 1765, the Reverend Dr. Chandler received a letter from Dr. Finley, President of the College in that province, relating the same experiment. It was read at the Royal Society, November 21st of that year, but not printed in the Transactions; perhaps because it was thought too strange to be true, and some ridicule might be apprehended, if any member should attempt to repeat it, in order to ascertain, or refute it. The following is a copy of that account.

"A worthy gentleman, who lives at a few miles distance, informed me, that in a certain small cove of a mill-pond, near his house, he was surprised to see the surface of the water blaze like inflamed spirits. I soon after went to the place, and made the experiment with the same success. The bottom of the creek was muddy, and when stirred up, so as to cause a considerable curl on the surface, and a lighted candle held within two or three inches of it, the whole surface was in a blaze, as instantly as the vapor of warm inflammable spirits, and continued, when strongly agitated, for the space of several seconds. It was at first imagined to be peculiar to that place; but upon trial it was soon found, that such a bottom in other places exhibited the same phenomenon. The discovery was accidentally made by one belonging to the mill."

I have tried the experiment twice here in England, but without success. The first was in a slow running water with a muddy bottom. The second in a stagnant water at the bottom of a deep ditch. Being some time employed in stirring this water, I ascribed an intermitting fever, which seized me a few days after, to my breathing too much of that foul air, which I stirred

up from the bottom, and which I could not avoid while I stooped, endeavouring to kindle it. The discoveries you have lately made, of the manner in which inflammable air is in some cases produced, may throw light on this experiment, and explain its succeeding in some cases, and not in others.

With the highest esteem and respect,
I am, dear Sir, your most obedient humble servant,
B. FRANKLIN.

AURORA BOREALIS.

*Suppositions and Conjectures towards forming an Hypothesis for its Explanation.**

1. Air heated by any means becomes rarefied and specifically *lighter* than other air in the same situation not heated.

2. Air being thus made lighter rises, and the neighbouring cooler, heavier air takes its place.

3. If in the middle of a room you heat the air by a stove, or pot of burning coals near the floor, the heated air will *rise* to the ceiling, spread there over the cooler air till it comes to the cold walls; there being condensed and made heavier, it *descends* to supply the place of that cool air which had moved towards the stove or fire, in order to supply the place of the heated air which had ascended from the space around the stove or fire.

4. Thus there will be a continual circulation of air

* First published in Mr. Vaughan's edition of the author's writings. Mr. Vaughan says, "If I mistake not, the paper was read at the Royal Academy of Sciences at Paris, at the meeting held immediately after Easter, 1779." —EDITOR.

in the room, which may be rendered visible by making a little smoke; for that smoke will rise and circulate with the air.

5. A similar operation is performed by nature on the air of the globe. Our atmosphere is of a certain height, perhaps at a medium miles. Above that height it is so rare as to be almost a vacuum. The air heated between the tropics is continually rising, and its place is supplied by northerly and southerly winds which come from those cool regions.

6. The light, heated air, floating above the cooler and denser, must spread northward and southward, and descend near the two poles, to supply the place of the cooler air which had moved towards the equator.

7. Thus a circulation of air is kept up in our atmosphere as in the room above mentioned.

8. That heavier and lighter air may move in currents of different and even opposite directions, appears sometimes by the clouds that happen to be in these currents, as plainly as by the smoke in the experiment above mentioned. Also in opening a *door* between two chambers, one of which has been warmed, by holding a candle near the top, near the bottom, and near the middle, you will find a strong current of warm air passing out of the warmed room *above*, and another of cool air entering it *below*, while in the middle there is little or no motion.

9. The great quantity of vapor rising between the tropics forms clouds, which contain much electricity.

Some of them fall in rain, before they come to the polar regions.

10. If the rain be received in an isolated vessel, the vessel will be electrified; for every drop brings down some electricity with it.

11. The same is done by snow and hail.

12. The electricity so descending in temperate climates, is received and imbibed by the earth.

13. If the clouds are not sufficiently discharged by this means, they sometimes discharge themselves suddenly by striking into the earth, where the earth is fit to receive their electricity.

14. The earth in temperate and warm climates is generally fit to receive it, being a good conductor.

15. A certain quantity of heat will make some bodies good conductors that will not otherwise conduct.

16. Thus wax rendered fluid, and glass softened by heat, will both of them conduct.

17. And water, though naturally a good conductor, will not conduct well when frozen into ice by a common degree of cold; not at all where the cold is extreme.

18. Snow falling upon frozen ground has been found to retain its electricity; and to communicate it to an isolated body, when after falling, it has been driven about by the wind.

19. The humidity, contained in all the equatorial clouds that reach the polar regions, must there be condensed and fall in snow.

20. The great cake of ice that eternally covers those regions may be too hard frozen to permit the electricity, descending with that snow, to enter the earth.

21. It will therefore be *accumulated upon that ice.*

22. The atmosphere being heavier in the polar regions, than in the equatorial, will there be lower; as well from that cause, as from the smaller effect of the centrifugal force; consequently the distance to the vacuum above the atmosphere will be less at the poles than elsewhere; and probably much less than the distance (upon the surface of the globe) extending from the pole to those latitudes in which the earth is so

thawed as to receive and imbibe electricity; the frost continuing to latitude 80, which is 10 degrees or 600 miles from the pole, while the height of the atmosphere there, of such density as to obstruct the motion of the electric fluid, can scarce be estimated above miles.

23. The *vacuum* above is a good conductor.

24. May not then the great quantity of electricity brought into the polar regions by the clouds, which are condensed there, and fall in snow, which electricity would enter the earth, but cannot penetrate the ice; may it not, I say *(as a bottle overcharged)* break through that low atmosphere and run along in the vacuum over the air towards the equator, diverging as the degrees of longitude enlarge, strongly visible where densest, and becoming less visible as it more diverges; till it finds a passage to the earth in more temperate climates, or is mingled with their upper air?

25. If such an operation of nature were really performed, would it not give all the appearances of an AURORA BOREALIS? See Plate XI. Fig. 1.

26. And would not the auroræ become more frequent *after the approach of winter;* not only because more visible in longer nights; but also because in summer the long presence of the sun may soften the surface of the great ice cake, and render it a conductor, by which the accumulation of electricity in the polar regions will be prevented?

27. The *atmosphere of the polar regions* being made more dense by the extreme cold, and all the moisture in that air being frozen, may not any great light arising therein, and passing through it, render its density in some degree visible during the night-time, to those who live in the rarer air of more southern latitudes? And would it not, in that case, although in itself a com-

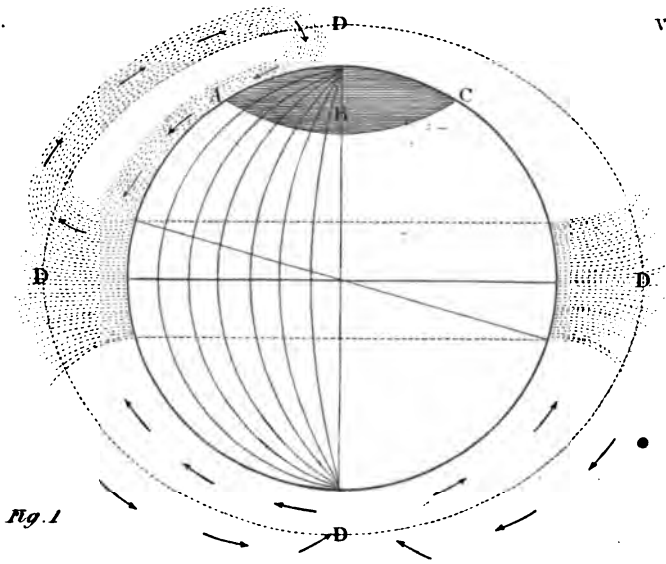
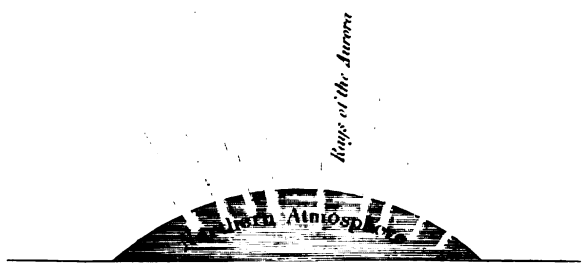


Fig. 1

The Arrows represent the general Currents of the Air.
A.B.C. the great Cake of Ice & Snow in the Polar Regions.
D.D.D.D. the Medium Height of the Atmosphere.
The Representation is made only for one Quarter and one Meridian of the Globe: but is to be understood the same for all the rest.



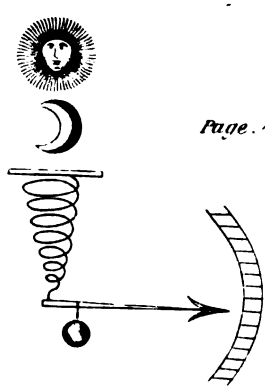


Fig. 2. *Page. 162.*

plete and full circle, extending perhaps ten degrees from the pole, appear to spectators so placed (who could see only a part of it) *in the form of a segment*, its chord resting on the horizon, and its arch elevated more or less above it, as seen from latitudes more or less distant, *darkish in color*, but yet sufficiently *transparent* to permit some stars to be seen through it?

28. The rays of electric matter issuing out of a body, diverge by mutually repelling each other, unless there be some conducting body near to receive them; and if that conducting body be at a greater distance, they will *first diverge*, and then *converge* in order to enter it. May not this account for some of the varieties of figure seen at times in the *motions* of the luminous matter of the auroræ; since it is possible, that, in passing over the atmosphere from the north, in all directions or meridians, towards the equator, the rays of that matter may find, in many places portions of cloudy region, or moist atmosphere under them, which (being in the natural or negative state) may be fit to receive them, and towards which they may therefore converge; and when one of those receiving bodies is more than saturated, they may *again* diverge from it, towards other surrounding masses of such humid atmosphere, and thus form the *crowns*, as they are called, and other figures, mentioned in the histories of this meteor?

29. If it be true, that the clouds which go to the polar regions carry thither the vapors of the equatorial and temperate regions, which vapors are condensed by the extreme cold of the polar regions, and fall in snow or hail; the winds which come from those regions ought to be generally dry, unless they gain some humidity by sweeping the ocean in their way; and, if I mistake not, the winds between the northwest and northeast

are for the most part dry, when they have continued some time.*

[In the Philosophical Transactions for 1774, p. 128, is a letter from Mr. J. S. Winn, to Dr. Franklin, stating, that since he had first made the observation concerning the south or southwest winds succeeding an aurora, he had found it invariably obtaining in twenty-three instances; and he adds in a note a fresh confirming instance. In reply, Dr. Franklin makes the following conjecture.]

The *aurora borealis*, though visible almost every night of clear weather in the more northern regions, and very high in the atmosphere, can scarce be visible in England but when the atmosphere is pretty clear of clouds for the whole space between us and those regions; and therefore are seldom visible there. This extensive clearness may have been produced by a long continuance of northerly winds. When the winds have long continued in one quarter, the return is often violent. Allowing the fact so repeatedly observed by Mr. Winn, perhaps this may account for the violence of the southerly winds, that soon follow the appearance of the aurora on our coasts.

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* In one of the copies of this paper, there is a line drawn across this last article. — W. T. F.

This paragraph is not contained in Mr. Vaughan's edition, and was probably not communicated to him by the author. — Editor.

FROM JOSEPH PRIESTLEY TO B. FRANKLIN.

Vegetation of Plants in Water.

Calne, 27 September, 1779.

DEAR SIR,

Though you are so much engaged in affairs of more consequence, I know it will give you some pleasure to be informed, that I have been exceedingly successful in the prosecution of my experiments since the publication of my last volume.

I have confirmed, explained, and extended my former observations on the purification of the atmosphere by means of vegetation; having just discovered that the *green matter* I treat of in my last volume is a vegetable substance, and then that other plants that grow wholly in water have the same property, all of them without exception imbibing impure air, and emitting it, as *excrementitious* to them, in a dephlogisticated state. That the source of this pure air is the impure air in the water is evident from all the plants giving only a certain quantity of air, in proportion to the water in which they are confined, and then giving more air in fresh water. I also find that the water before the plants have been confined in it yields impure air, and afterwards pure air.

From these observations I conclude, that the reason why my sprigs of mint sometimes failed to purify air, was their not being always *healthy* in a confined state. Whereas these water plants are as much at their ease in my jars as in the open pond.

I have made many other new observations, but they are chiefly of a chemical nature, and not worth making the subject of a letter; though, when you see an account of them in my next publication, I flatter myself you will think some of them curious and important.

As the expense of my experiments is necessarily considerable, Dr. Fothergill (who, in a very obliging manner, interests himself much in them) has, of his own motion, engaged a few of his friends to contribute about forty pounds per annum to assist me in defraying the expense. Indeed, without this assistance, I must have desisted altogether. While I had but a small family, I suffered myself to be drawn by my success into expenses, that now appear to have been rather imprudent.

I have been made very happy by the communication of your very ingenious paper on the *Aurora Borealis*, and by several accounts of the good state of your health and spirits. May this long continue.

Yours most sincerely,

JOSEPH PRIESTLEY.

FROM PIETRO TURINI TO B. FRANKLIN.

Communicating Intelligence of a new Work on the Method of Erecting Conductors of Lightning.

TRANSLATION.

Venice, 18 September, 1780.

SIR,

For a long time a most brilliant reputation has made your name resound through all parts of the world. All Europe admires you; and there is no one more deeply penetrated than myself with the esteem and veneration, which are due to you for so many reasons. Your works, which I have read with the greatest avidity, and from which I have drawn the most useful instructions, have made me comprehend the immense extent of your rare knowledge and of your talents.

The theory of electricity has fixed, and will fix for ever, the attention of men. So interesting a dis-

covery, reserved to the sublimity of your genius, must necessarily introduce a sensible difference in the explanation of the principal phenomena of nature, and of many of its laws. Indeed the greater part of the aërial and terrestrial phenomena are explained by the aid of the electric fire with facility and convincing precision.

The useful theory of conductors, a very important fruit of your studies, makes every day some new progress. The unanimous consent of men proves the truth of the system, and its universal adoption in practice shows how generally they are persuaded of the advantages which result from it.

The method at present used throughout the world, in erecting conductors upon edifices, is essentially the same; and if there is any difference, it consists of some trifles, and more or less carefulness, which depend very often upon the ideas of those, who have the charge of their erection. Yet it must be confessed, that this method has some essential faults, which may sooner or later be the cause of irreparable losses. Powder magazines especially remain exposed to the dreadful attacks of lightning, and thus we still have sad consequences to fear from them.

You will find, Sir, in the little work accompanying this, which I have the honor to dedicate to you, an exposition of the reasons which have convinced me of the insufficiency of the precautions generally adopted. I hope I have found the means of perfectly sheltering these very important edifices from lightning, and given generally more security to other buildings. The progress of the human mind is very rarely rapid. On the contrary it advances by small degrees, and men of genius resembling yourself are rare, who can extend their views to the utmost, and fix the true principles of

an important theory, like that of which you are the author. I have done nothing more than make a precise application of the best ascertained rules to the most careful practice. It would be mere rashness to pretend much, after your discoveries.

"Da lunge il seguo, e sue vestigie adoro."

I am anxious to submit this essay to your experience, and I shall be much flattered, if it is such as to merit your approbation. It is written in Italian, with the view of instructing my own countrymen. The importance of the matter has emboldened me to take the liberty to send it to you, such as it is. At least I shall have the satisfaction of having seized this opportunity of expressing to you the sentiments of admiration, and of respect, with which I have the honor to be,

Sir, &c.

PIETRO TURINI.

TO EDWARD NAIRNE, OF LONDON.

Proposing a slowly sensible Hygrometer for certain Purposes.

READ AT A MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY, JANUARY 26TH, 1786.

Passy, near Paris, 13 November, 1780.

SIR,

The qualities hitherto sought in a hygrometer, or instrument to discover the degrees of moisture and dryness in the air, seem to have been, an aptitude to receive humidity readily from a moist air, and to part with it as readily to a dry air. Different substances have been found to possess more or less of this quality; but, when we shall have found the substance that has it in the greatest perfection, there will still remain some

uncertainty in the conclusions to be drawn from the degree shown by the instrument, arising from the actual state of the instrument itself as to heat and cold. Thus, if two bottles or vessels of glass or metal being filled, the one with cold and the other with hot water, are brought into a room, the moisture of the air in the room will attach itself in quantities to the surface of the cold vessel; while, if you actually wet the surface of the hot vessel, the moisture will immediately quit it, and be absorbed by the same air. And thus, in a sudden change of the air from cold to warm, the instrument remaining longer cold may condense and absorb more moisture, and mark the air as having become more humid than it is in reality, and the contrary in a change from warm to cold.

But, if such a suddenly changing instrument could be freed from these imperfections, yet, when the design is to discover the different degrees of humidity in the air of different countries, I apprehend the quick sensibility of the instrument to be rather a disadvantage; since, to draw the desired conclusions from it, a constant and frequent observation day and night in each country will be necessary for a year or years, and the mean of each different set of observations is to be found and determined. After all which, some uncertainty will remain respecting the different degrees of exactitude with which different persons may have made and taken notes of their observations.

For these reasons I apprehend, that a substance, which, though capable of being distended by moisture and contracted by dryness, is so slow in receiving and parting with its humidity, that the frequent changes in the atmosphere have not time to affect it sensibly, and which therefore should gradually take nearly the medium of all those changes and preserve it constantly,

would be the most proper substance of which to make such an hygrometer.

Such an instrument, you, my dear Sir, though without intending it, have made for me; and I, without desiring or expecting it, have received from you. It is therefore with propriety that I address to you the following account of it; and the more, as you have both a head to contrive and a hand to execute the means of perfecting it. And I do this with greater pleasure, as it affords me the opportunity of renewing that ancient correspondence and acquaintance with you, which to me was always so pleasing and so instructive.

You may possibly remember, that, in or about the year 1758, you made for me a set of artificial magnets, six in number, each five inches and a half long, half an inch broad, and one eighth of an inch thick. These, with two pieces of soft iron, which together equalled one of the magnets, were enclosed in a little box of mahogany wood, the grain of which ran with, and not across, the length of the box; and the box was closed by a little shutter of the same wood, the grain of which ran across the box; and the ends of this shutting piece were bevelled so as to fit and slide in a kind of dovetail groove when the box was to be shut or opened.

I had been of opinion, that good mahogany wood was not affected by moisture so as to change its dimensions, and that it was always to be found as the tools of the workman left it. Indeed the difference at different times in the same country is so small as to be scarcely in a common way observable. Hence the box, which was made so as to allow sufficient room for the magnets to slide out and in freely, and, when in, afforded them so much play that by shaking the box one could make them strike the opposite sides alternately, continued in the same state all the time I remained

in England, which was four years, without any apparent alteration. I left England in August, 1762, and arrived at Philadelphia in October the same year. In a few weeks after my arrival, being desirous of showing your magnets to a philosophical friend, I found them so tight in the box, that it was with difficulty I got them out; and constantly, during the two years I remained there, viz. till November, 1764, this difficulty of getting them out and in continued. The little shutter too, as wood does not shrink lengthways of the grain, was found too long to enter its grooves, and, not being used, was mislaid and lost; and I afterwards had another made that fitted.

In December, 1764, I returned to England, and after some time I observed that my box was become full big enough for my magnets, and too wide for my new shutter; which was so much too short for its grooves, that it was apt to fall out; and to make it keep in, I lengthened it by adding to each end a little coat of sealing-wax.

I continued in England more than ten years, and, during all that time, after the first change, I perceived no alteration. The magnets had the same freedom in their box, and the little shutter continued with the added sealing-wax to fit its grooves, till some weeks after my second return to America.

As I could not imagine any other cause for this change of dimensions in the box, when in the different countries, I concluded, first generally that the air of England was moister than that of America. And this I supposed an effect of its being an island, where every wind that blew must necessarily pass over some sea before it arrived, and of course lick up some vapor. I afterwards indeed doubted whether it might be just only so far as related to the city of London,

where I resided; because there are many causes of moisture in the city air, which do not exist to the same degree in the country; such as the brewers' and dyers' boiling caldrons, and the great number of pots and tea-kettles continually on the fire, sending forth abundance of vapor; and also the number of animals who by their breath continually increase it; to which may be added, that even the vast quantity of sea coals, burnt there, do in kindling discharge a great deal of moisture.

When I was in England, the last time, you also made for me a little achromatic pocket telescope; the body was brass, and it had a round case (I think of thin wood) covered with shagreen. All the while I remained in England, though possibly there might be some small changes in the dimensions of this case, I neither perceived nor suspected any. There was always comfortable room for the telescope to slip in and out. But soon after I arrived in America, which was in May, 1775, the case became too small for the instrument; it was with much difficulty and various contrivances that I got it out, and I could never after get it in again, during my stay there, which was eighteen months. I brought it with me to Europe, but left the case as useless, imagining that I should find the continental air of France as dry as that of Pennsylvania, where my magnet-box had also returned a second time to its narrowness, and pinched the pieces, as heretofore, obliging me, too, to scrape the sealing-wax off the ends of the shutter.

I had not been long in France, before I was surprised to find, that my box was become as large as it had always been in England, the magnets entered and came out with the same freedom, and, when in, I could rattle them against its sides; this has continued to be the case without sensible variation. My habi-

tation is out of Paris distant almost a league, so that the moist air of the city cannot be supposed to have much effect upon the box. I am on a high, dry hill, in a free air, as likely to be dry as any air in France. Whence it seems probable that the air of England in general may, as well as that of London, be moister than the air of America, since that of France is so, and in a part so distant from the sea.

The greater dryness of the air in America appears from some other observations. The cabinet work formerly sent us from London, which consisted in thin plates of fine wood glued upon fir, never would stand with us; the vaneering, as those plates are called, would get loose and come off; both woods shrinking, and their grains often crossing, they were for ever cracking and flying. And in my electrical experiments there, it was remarkable, that a mahogany table, on which my jars stood under the prime conductor to be charged, would often be so dry, particularly when the wind had been some time at northwest, which with us is a very drying wind, as to isolate the jars, and prevent their being charged till I had formed a communication between their coatings and the earth. I had a like table in London, which I used for the same purpose all the time I resided there; but it was never so dry as to refuse conducting the electricity.

Now what I would beg leave to recommend to you, is, that you would recollect, if you can, the species of mahogany of which you made my box, for you know there is a good deal of difference in woods that go under that name; or, if that cannot be, that you would take a number of pieces of the closest and finest grained mahogany that you can meet with, plane them to the thinness of about a line, and the width of about two inches across the grain, and fix each of the pieces in some instrument that you can contrive, which will

permit them to contract and dilate, and will show, in sensible degrees, by a movable hand upon a marked scale, the otherwise less sensible quantities of such contraction and dilatation. If these instruments are all kept in the same place while making, and are graduated together while subject to the same degrees of moisture or dryness, I apprehend you will have so many comparable hygrometers, which, being sent into different countries, and continued there for some time, will find and show there the mean of the different dryness and moisture of the air of those countries, and that with much less trouble than by any hygrometer hitherto in use.* With great esteem, I am, dear Sir,

Your most obedient and most humble servant,

B. FRANKLIN.

TO THE MARQUIS TURGOT.†

On a new-invented Stove.

Passy, 1 May, 1781.

SIR,

I did intend, when in London, to have published a pamphlet, describing the new stove you mention, and for that purpose had a plate engraved, of which I send you an impression. But I have since been too much engaged in affairs to execute that intention. Its principle is that of a siphon reversed, operating on air in a manner somewhat similar to the operation of the common siphon on water. The funnel of the chimney is the longer leg, the vase is the shorter; and as, in the common siphon, the weight of water in the longer

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* For the description of a hygrometer constructed by Mr. Nairne, in conformity with these directions, see his letter under the date of December 2d, 1783. — EDITOR.

† Brother to the celebrated French financier and philosopher of the same name. — EDITOR.

leg is greater than that in the shorter leg; and thus in descending permits the water in the shorter leg to rise, by the pressure of the atmosphere; so in this aërial siphon, the levity of the air in the longer leg being greater than that in the shorter, it rises and permits the pressure of the atmosphere to force that in the shorter to descend. This causes the smoke to descend also, and in passing through burning coals it is kindled into flame, thereby heating more the passages in the iron box whereon the vase which contains the coals is placed; and retarding at the same time the consumption of the coals.

On the left hand of the engraving you see the machine put together and placed in a niche built for it in a common chimney.* On the right hand the parts (except the vase) are shown separately. If you should desire a more particular explanation, I will give it to you *vivâ voce*, whenever you please. I think with you, that it is capable of being used to advantage in our kitchens, if one could overcome the repugnance of cooks to the using of new instruments and new methods. With great respect,

B. FRANKLIN.

TO FELIX VICQ D'AZYR.†

On the Long Retention of Infection in Dead Bodies after Sepulture.

Passy, 20 July, 1781.

SIR,

I received the letter you some time since did me the honor of writing to me, accompanied with a

* Probably referring to Plate XV. in the present volume. — EDITOR.

† Vicq d'Azyr was the physician to the Queen of France, and celebrated for his skill in medicine and his knowledge of science. His works were published in six volumes. — EDITOR.

number of the pieces, that were distributed at the last public meeting of the Royal Society of Medicine. I shall take care to forward them to different parts of America, as desired. Be pleased to present my thanks to the Society for the copy sent me of the curious and useful reports relating to the sepulture in the island of Malta. I should be glad of another copy, if it can be spared, being desirous of sending one to each of the philosophical societies in America.

With respect to the length of time during which the power of infection may be contained in dead bodies, which is considered in that report, I would mention to you three facts, which, though not all of equal importance or weight, yet methinks it may be well to preserve a memorandum of them, that such observations may be made when occasion offers, as are proper to confirm or invalidate them.

While I resided in England, I read in a newspaper, that in a country village at the funeral of a woman whose husband had died of the smallpox thirty years before, and whose grave was dug so as to place her by his side, the neighbours attending the funeral were offended with the smell arising out of the grave, occasioned by a breach in the husband's old coffin, and twenty-five of them were in a few days taken ill with that distemper, which before was not in that village or its neighbourhood, nor had been for the number of years above mentioned.

About the years 1763 or 1764, several physicians of London, who had been present from curiosity at the dissection of an Egyptian mummy, were soon after taken ill of a malignant fever, of which they died. Opinions were divided on this question. It was thought by some that the fever was caused by infection from the mummy; in which case the disease it

died of must have been embalmed as well as the body. Others who considered the length of time; at least two thousand years, since that body died, and also that the embalming must be rather supposed to destroy the power of infection, imagined the illness of these gentle men must have had another original.

About the year 1773, the captain of a ship, which had been at the island of Teneriffe, brought from thence the dried body of one of the ancient inhabitants of that island, which must have been at least three hundred years old, that custom of drying the dead there having been so long discontinued. Two members of the Royal Society went to see that body. They were half an hour in a small close room with it, examining it very particularly. The next day they were both affected with a singularly violent *cold*,* attended with uncommon circumstances, which continued a long time. On comparing together the particulars of their disorder, they agreed in suspecting that possibly some effluvia from the body might have been the occasion of that disorder in them both; perhaps they were mistaken. But, as we do not yet know with certainty how long the power of infection may in some bodies be retained, it seems well in such cases to be cautious till farther light shall be obtained.

I wish it were in my power to contribute more essentially in advancing the good work the Society are so laudably engaged in. Perhaps some useful hints may be extracted from the enclosed paper of Mr. Small's.† It is submitted to your judgment; and, if you should find any thing in it worthy of being communicated to the Society, and of which extracts may

* *Cold* is a general name given by the English to all sorts of rheums and catarrhs.

† See the article on *Ventilation*, by Mr. Small, above, p. 307.

be useful if printed in the Memoirs, it will be a pleasure to me; who am, with great esteem and respect, Sir, &c.

B. FRANKLIN.

P. S. July 24. Since writing the above, I have met with the following article in the *Courier de l'Europe* of the 13th instant, viz.

Extrait d'une Lettre d'Edimbourg, en date du 30 Juin.

"J'apprends par une personne qui vient de Montrose, que la fièvre épidémique qui s'est manifestée il y a quelque tems dans le Méarns, désole encore aujourd'hui ce voisinage avec tant de violence qu'un de ses amis a été invité à assister à quinze enterrements dans un seul jour. On dit que cette maladie doit son origine à la folle curiosité de quelques paysans, qui, à la Chandeleur dernière, exhumèrent quelques personnes mortes de la peste dans le siècle précédent, et qu'on avoit enterrées dans le Moss de *Arnhall*. Ce qui est arrivé à la famille de M. Robert Aikenhead est singulièrement malheureux; vers le milieu du mois dernier il a été attaqué de cette contagion, et elle s'est communiquée au reste de sa famille, consistant en neuf personnes, dont deux sont mortes ainsi que lui, et le reste n'est pas sans danger."

Translation.

Extract of a Letter from Edinburgh, dated June 30th.

"I understand by a person just returned from Montrose, that the epidemic fever which has made its appearance in the county of Mearns, ravages that neighbourhood with such violence, that one of his friends was invited to attend fifteen funerals on the same day.

It is said that this malady originated in the ill-judged curiosity of some country people, who, at Candlemas last, opened the graves of some persons, who had died of the plague in the preceding century, and who had been buried in the Moss of Arnhall. The circumstances which have happened in the family of Mr. Robert Aikenhead are singularly unfortunate; about the middle of last month he took the infection, which was communicated to the rest of his family, consisting of nine persons; two of whom, together with himself, are dead, and the others not out of danger."

FROM FELIX VICQ D'AZYR TO B. FRANKLIN.

TRANSLATION.

Paris, 23 August, 1781.

Sir,

I have presented to the Royal Society of Medicine, conformably to your intentions, the memoir by Mr. Small translated from the English, upon the method of ventilating the interior of hospitals, together with your letter which accompanied it.

The Society has attended to the reading of both of these pieces. Your letter contains some very important observations, which are worth preserving, upon the time necessary for the decay of bodies buried in the ground. The Society is much flattered that you have approved its report upon the dangers of interment. You have requested some copies of it, which I have the honor to send you. The Society desired to hear the entire reading of the observations of Mr. Small, and was highly pleased with them. We have there found your great principles of physics. A detailed report will be made upon this subject, in

which the most ample justice will be done to the author. The Society begs you to accept their thanks, and to communicate them to Mr. Small, whose paper we have listened to with so much pleasure.

I am, Sir, &c.,

VICQ D'AZYR.

TO JOHN INGENHOUSZ.

On Conductors of Heat.

Passy, 2 October, 1781.

It is a long time, my dear friend, since I have had the pleasure of writing to you. I have postponed it too often from a desire of writing a good deal on various subjects, which I could not find sufficient time to think of properly. Your experiments *on the conducting of heat* was one subject; the finishing my *remarks on the stroke of lightning in Italy** was another. Then I was taken ill with a severe fit of the gout soon after you left us, which held me near three months, and put my business and correspondence so far behind-hand, that I was long in getting it up again. Add to this, that I find indolence increases with age, and that I have not near the activity I formerly had. But I cannot afford to lose your correspondence, in which I have always found so much pleasure and instruction. I now force myself to write, and I fancy this letter will be long.

I have now before me your several favors of December 5th, 1780, February 7th, April 7th, May 23d, and August 29th, 1781. I was glad to find by the first, that you enjoyed a good state of health, and

* See *An Attempt to explain the Effects of Lightning on the Steeple of a Church in Cremona*; Vol. V. p. 467.

that you had leisure to pursue your philosophical inquiries. I wish you that continued success, which so much industry, sagacity, and exactness in making experiments, have a right to expect. You will have much immediate pleasure by that success, and in time great reputation. But for the present the reputation will be given grudgingly, and in as small a quantity as possible, mixed too with some mortification. One would think that a man so laboring disinterestedly for the good of his fellow creatures, could not possibly by such means make himself enemies; but there are minds who cannot bear that another should distinguish himself even by greater usefulness; and though he demands no profit, nor any thing in return but the good will of those he is serving, they will endeavour to deprive him of that, first by disputing the truth of his experiments, then their utility; and, being defeated there, they finally dispute his right to them, and would give the credit of them to a man that lived three thousand years ago, or at three thousand leagues distance, rather than to a neighbour or even a friend. Go on, however, and never be discouraged. Others have met with the same treatment before you, and will after you. And, whatever some may think and say, it is worth while to do men good, for the self-satisfaction one has in the reflection.

Your account of the experiments you made with the wires gave me a great deal of pleasure. I have shown it to several persons here, who think it exceedingly curious. If you should ever repeat those experiments, I wish your attention to one circumstance. I think it possible, that, in dipping them into the wax, and taking them out suddenly, the metal which attracts heat most readily may chill and draw out with it a thicker coat of wax; and this thicker coat might,

in the progress of the experiment, be longer melting. They should therefore be kept so long in the wax, as to be all well and equally heated. Perhaps you may thus find the progress of heat in the silver quicker and greater. I think, also, that, if the hot oil in which you dipped the ends was not stagnant, but in motion, the experiment would be more complete, because the wire which quickest diminishes the heat of the oil next to it, finds soonest the difficulty of getting more heat from the oil farther distant, which depends on the nature of the oil as a conductor of heat, that which is already cooled interfering between the hotter oil and the wire. In reversing the experiment also, to try which of the metals cools fastest, I think the wires should be dipped in *running* cold water; for, when stagnant, the hot wires, by communicating heat to the water that is near them, will make it less capable of receiving more heat; and, as the metals which communicate their heat most freely and readily will soonest warm the water round them, the operation of such metals may therefore soonest stop; not because they naturally longer withhold their heat, but because the water near them is not in a state to receive it. I do not know that these hints are founded; I suggest them only as meriting a little consideration. Every one is surprised that the progress of the heat seems to have no connexion with the gravity or the levity of the metals.

B. FRANKLIN.

An Account of Toads found enclosed in Solid Stone.

At Passy, near Paris, April 6th, 1782, being with M. de Chaumont, viewing his quarry, he mentioned to me, that the workmen had found a living toad shut up in the stone. On questioning one of them, he told us, they had found four in different cells which had no communication; that they were very lively and active when set at liberty; that there was in each cell some loose, soft, yellowish earth, which appeared to be very moist. We asked, if he could show us the parts of the stone that formed the cells. He said, No; for they were thrown among the rest of what was dug out, and he knew not where to find them. We asked, if there appeared any opening by which the animal could enter. He said, No. We asked, if, in the course of his business as a laborer in quarries, he had often met with the like. He said, Never before. We asked, if he could show us the toads. He said, he had thrown two of them up on a higher part of the quarry, but knew not what became of the others.

He then came up to the place where he had thrown the two, and, finding them, he took them by the foot, and threw them up to us, upon the ground where we stood. One of them was quite dead, and appeared very lean; the other was plump and still living. The part of the rock where they were found, is at least fifteen feet below its surface, and is a kind of limestone. A part of it is filled with ancient sea-shells, and other marine substances. If these animals have remained in this confinement since the formation of the rock, they are probably some thousands of years old. We have put them in spirits of wine, to preserve their bodies a little longer. The

workmen have promised to call us, if they meet with any more, that we may examine their situation. Before a suitable bottle could be found to receive them, that which was living when we first had them appeared to be quite dead and motionless; but being in the bottle, and the spirits poured over them, he flounced about in it very vigorously for two or three minutes, and then expired.

It is observed, that animals who perspire but little, can live long without food; such as tortoises, whose flesh is covered with a thick shell, and snakes, who are covered with scales, which are of so close a substance as scarcely to admit the passage of perspirable matter through them. Animals that have open pores all over the surface of their bodies, and live in air which takes off continually the perspirable part of their substance, naturally require a continual supply of food to maintain their bulk. Toads shut up in solid stone, which prevents their losing any thing of their substance, may perhaps for that reason need no supply; and being guarded against all accidents, and all the inclemencies of the air and changes of the seasons, are, it seems, subject to no diseases, and become as it were immortal.

B. FRANKLIN.*

* The following copy of a letter from Sir John Pringle to Mr. A. Small, was annexed to the above account, in Dr. Franklin's papers. — W. T. F.

"Minorca, 25 April, 1780.

"SIR,

"Last year I had the honor to inform you, that two of those large moths called Muskitoe Hawks, which appear about September, and disappear about the beginning of December, lived seventy-one days after I had cut their heads off with a pair of scissors.

"The last autumn, I made the same experiment upon several, keeping them under separate glasses, in a closet, where there was no fire. The most of them lived different periods, from three, to sixty and seventy days. Those which exceeded that number of days were four, viz. one from the 30th of October to the 21st of January, eighty-three days; one from the

TO THE ABBÉ SOULAVIE.*

On the Theory of the Earth.

READ AT A MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY, NOVEMBER 21ST, 1788.

Passy, 22 September, 1782.

SIR,

I return the papers with some corrections. I did not find coal mines under the calcareous rock in Derbyshire. I only remarked, that, at the lowest part of that rocky mountain which was in sight, there were oyster shells mixed in the stone; and part of the high county of Derby being probably as much above the level of the sea, as the coal mines of Whitehaven were below it, seemed a proof, that there had been a great *bouleversement* in the surface of that island, some part of it having been depressed under the sea, and other parts, which had been under it, being raised above it. Such changes in the superficial parts of the globe seemed to me unlikely to happen, if the earth were solid to the centre. I therefore imagined, that the internal parts might be a fluid more dense, and of

12th of December to the 21st of April, one hundred and thirty-one days; and one from the 24th of October to the 15th of April, one hundred and seventy-four days. As they are very active, and covered with a sort of plumage, which makes it difficult to cut their heads off, without bruising or otherwise injuring the body, I imagine that may partly be the reason of their living different periods; and if, after the operation, any glutinous liquor proceeded from the body, that moth would die soon.

"I put several under glasses, without cutting off their heads, none of which lived many days.

"I am, Sir, with great esteem, your most obedient and most humble servant,

"JOHN PRINGLE."

* Occasioned by his sending me some notes he had taken, of what I had said to him in conversation on the Theory of the Earth. I wrote it to set him right in some points wherein he had mistaken my meaning.—*Note by the Author.*

greater specific gravity than any of the solids we are acquainted with, which therefore might swim in or upon that fluid. Thus the surface of the globe would be a shell, capable of being broken and disordered by the violent movements of the fluid on which it rested. And as air has been compressed by art, so as to be twice as dense as water, in which case, if such air and water could be contained in a strong glass vessel, the air would be seen to take the lowest place, and the water to float above and upon it; and as we know not yet the degree of density to which air may be compressed, and M. Amontons calculated, that, its density increasing as it approached the centre in the same proportion as above the surface, it would, at the depth of leagues, be heavier than gold, possibly the dense fluid occupying the internal parts of the globe might be air compressed. And as the force of expansion in dense air when heated is in proportion to its density, this central air might afford another agent to move the surface, as well as be of use in keeping alive the subterraneous fires; though, as you observe, the sudden rarefaction of water coming into contact with those fires, may also be an agent sufficiently strong for that purpose, when acting between the incumbent earth and the fluid on which it rests.

If one might indulge imagination, in supposing how such a globe was formed, I should conceive, that, all the elements in separate particles being originally mixed in confusion, and occupying a great space, they would (as soon as the almighty fiat ordained gravity, or the mutual attraction of certain parts, and the mutual repulsion of others, to exist) all move to their common centre; that the air being a fluid whose parts repel each other, though drawn to the common centre by their gravity, would be densest towards the centre, and rarer as more remote; consequently all matters

lighter than the central parts of that air, and immersed in it, would recede from the centre, and rise till they arrived at that region of the air which was of the same specific gravity with themselves, where they would rest ; while other matter, mixed with the lighter air, would descend, and the two meeting would form the shell of the first earth, leaving the upper atmosphere nearly clear. The original movement of the parts towards their common centre would naturally form a whirl there; which would continue upon the turning of the new-formed globe upon its axis, and the greatest diameter of the shell would be in its equator. If by any accident afterwards, the axis should be changed, the dense internal fluid, by altering its form, must burst the shell, and throw all its substance into the confusion in which we find it.

I will not trouble you at present with my fancies concerning the manner of forming the rest of our system. Superior beings smile at our theories, and at our presumption in making them. I will just mention, that your observation of the ferruginous nature of the lava, which is thrown out from the depths of our volcanoes, gave me great pleasure. It has long been a supposition of mine, that the iron contained in the surface of the globe has made it capable of becoming, as it is, a great magnet; that the fluid of magnetism perhaps exists in all space; so that there is a magnetical north and south of the universe, as well as of this globe, and that, if it were possible for a man to fly from star to star, he might govern his course by the compass; that it was by the power of this general magnetism this globe became a particular magnet. In soft or hot iron, the fluid of magnetism is naturally diffused equally; when within the influence of the magnet it is drawn to one end of the iron, made denser

there and rarer at the other. While the iron continues soft and hot, it is only a temporary magnet; if it cools or grows hard in that situation, it becomes a permanent one, the magnetic fluid not easily resuming its equilibrium. Perhaps it may be owing to the permanent magnetism of this globe, which it had not at first, that its axis is at present kept parallel to itself, and not liable to the changes it formerly suffered, which occasioned the rupture of its shell, the submersions and emersions of its lands, and the confusion of its seasons. The present polar and equatorial diameters differing from each other near ten leagues, it is easy to conceive, in case some power should shift the axis gradually, and place it in the present equator, and make the new equator pass through the present poles, what a sinking of the waters would happen in the present equatorial regions, and what a rising in the present polar regions; so that vast tracts would be discovered, that now are under water, and others covered, that are now dry, the water rising and sinking in the different extremes near five leagues. Such an operation as this possibly occasioned much of Europe, and among the rest this Mountain of Passy on which I live, and which is composed of limestone, rock, and seashells, to be abandoned by the sea, and to change its ancient climate, which seems to have been a hot one.

The globe being now become a perfect magnet, we are, perhaps, safe from any change of its axis. But we are still subject to the accidents on the surface, which are occasioned by a wave in the internal ponderous fluid; and such a wave is producible by the sudden violent explosion you mention, happening from the junction of water and fire under the earth, which not only lifts the incumbent earth that is over the explosion, but, impressing with the same force the fluid under it, creates a wave, that may run a thousand

leagues, lifting, and thereby shaking, successively, all the countries under which it passes. I know not, whether I have expressed myself so clearly, as not to get out of your sight in these reveries. If they occasion any new inquiries, and produce a better hypothesis, they will not be quite useless. You see I have given a loose to imagination; but I approve much more your method of philosophizing, which proceeds upon actual observation, makes a collection of facts, and concludes no farther than those facts will warrant. In my present circumstances, that mode of studying the nature of the globe is out of my power, and therefore I have permitted myself to wander a little in the wilds of fancy. With great esteem,

I have the honor to be, Sir, &c.

B. FRANKLIN.

P. S. I have heard, that chemists can by their art decompose stone and wood, extracting a considerable quantity of water from the one, and air from the other. It seems natural to conclude, from this, that water and air were ingredients in their original composition; for men cannot make new matter of any kind. In the same manner may we not suppose, that, when we consume combustibles of all kinds, and produce heat or light, we do not create that heat or light; but only decompose a substance, which received it originally as a part of its composition? Heat may be thus considered as originally in a fluid state; but, attracted by organized bodies in their growth, becomes a part of the solid. Besides this, I can conceive, that, in the first assemblage of the particles of which this earth is composed, each brought its portion of the loose heat that had been connected with it, and the whole, when pressed together, produced the internal fire that still subsists.

FROM JOHN INGENHOUSZ TO B. FRANKLIN.

Experiment for burning Wire.

Vienna, 29 November, 1782.

——I send you a wire, folded up in a spiral, which you must arrange as you see the wire on the figure I join to it. Leave at the end of it the bit of agaric. When you have a bottle of white glass, of about a pint or more, filled with good dephlogisticated air, take away the cork, and clap your thumb of the left hand on the orifice. In the mean time, turn for a moment the bit of agaric in the flame of a candle, till you see it is everywhere kindled. Hold the wire in the right hand till you see that the piece of agaric gives no more smoke, but is become quite a red hot coal. Then thrust it immediately in the bottle, more than half its depth, and keep it there till it is consumed. The coal of the agaric will increase in heat so as to set fire to the end of the wire. The flame will slowly run up, and the melted metal will now and then fall to the bottom of the glass, and will crack it, if you leave water in the bottle. You will be highly delighted with the experiment. My bottles for that purpose have a metallic bottom. You have only to send an empty bottle of flint glass, with a good cork stopper upon it, to M. Sigau de la Fond, who lives in the Rue St. Jaques, in the neighbourhood of M. le Beguer. He has always a good deal of dephlogisticated air ready, and will be glad to let you have as much as you please. You must remind him not to leave more water in the bottle, than is sufficient to keep the cork moist, for the motion of water with this air spoils the air.

J. INGENHOUSZ.

FROM EDWARD NAIRNE TO B. FRANKLIN.

A Hygrometer on a New Construction. — M. De Luc's Hygrometer.

London, 2 December, 1783.

DEAR SIR,

I received your favors. The book and prints, which Mr. Argand was so obliging as to deliver, I have since sent to Sir Joseph Banks, agreeably to your request. I am very much obliged to you for your observations on the alteration of the wood of the box belonging to the magnets. Since I received your favor, I have been endeavouring to make as simple an instrument as possible for a hygrometer, a drawing of which is annexed.

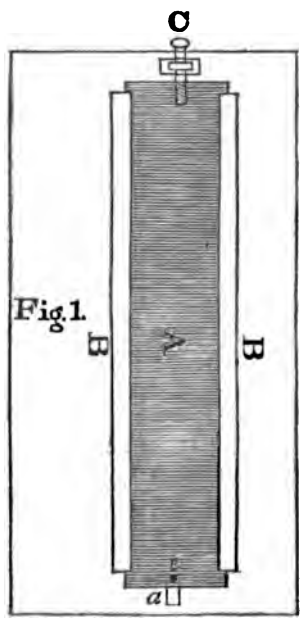

Fig. 1.

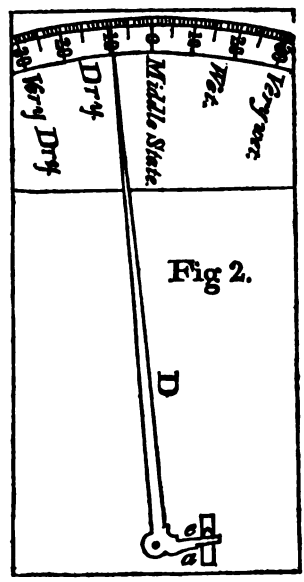

Fig 2.

In Fig. 1, *A* is a piece of wood about twelve inches long and two inches broad, cut crosswise the grain of

the wood, which slides freely between the two pieces of wood, *B, B,* forming grooves for it. *C* is a screw for adjusting the piece of wood, *A,* so that the index may point to the proper division when first made. In Fig. 2, *a* is a slit to admit the pin *e* to move freely, which pin, by being fast in the piece of wood, *A,* moves with it as it shortens or lengthens, and, by pressing against the short end of the index, *D,* causes it to move up or down according as the weather is moist or dry, which is shown on the divided arch, at the upper end of the instrument.*

If you should at your leisure consider it, and if you find that it is not adequate to the purpose wanted, I should esteem it a great favor if you would inform me of it, or if any alterations or additions can be added to it.†

I was the day before yesterday at Windsor, where M. De Luc showed me a hydrometer. It was made of a thin piece of whalebone, about nine inches long, and was kept straight by a line fastened to it going over a pulley, the other end of which line was fastened to a spiral spring. He observed, that it was the only substance he had ever met with, that would always return to the same length when soaked in water. It altered in its length about one inch from extreme moisture to extreme dryness.

I sent you some time ago the account of shortening wire by lightning, which I hope you have received.

I am, dear Sir, your most obliged, &c.

EDWARD NAIRNE.

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* It is to be observed, that Fig. 1. is the back part of the instrument, or a thin box upon which is placed Fig. 2, as a cover, being the face of the instrument. — EDITOR.

† See the letter, containing directions for making this hygrometer above, p. 426.

TO DAVID RITTENHOUSE.

On the Comet seen in Yorkshire.

Passy, 15 December, 1783.

SIR,

All astronomical news that I receive, I think it my duty to communicate to you. The following is just come to hand, in a letter from the President of the Royal Society, dated at London the 9th instant.

"A miserable comet made its appearance to Mr. Nathan Pigot, in his observatory at Yorkshire, on the 19th past, and the weather has been so hazy in the evenings that it has scarce been observed since. It was on the 19th

	h.	m.	Right Ascen.	North Dec.
at	11	15	41 0 0	3° 10′
"On the 20th	10	54	40 0 0	4 32

"On the 21st it was seen in the place where it was expected; but the night was too hazy to observe it.

"It appears like a nebula, with a diameter of about two minutes of a degree; the nucleus faint. It is seen with difficulty when the wires of the instrument are illuminated, but is not visible with an open glass."

Mr. Pigot.

"Nov. 29th. It was seen near the chin of Aries, and appeared like a nebulous star; as there was some moonlight, it was difficult to find it.

"Dec. 1st. It was removed near the preceding eye of Aries; but, conceiving other astronomers, who had fixed instruments, have noted its place, he has not calculated the distance from any known star."

Mr. Herschell.

With great esteem, I have the honor to be, &c.

B. FRANKLIN.

TO JOHN INGENHOUSZ.

On Balloons, and their Probable Importance.

Passy, 16 January, 1784.

DEAR FRIEND,

I have this day received your favor of the 2d instant. Every information in my power, respecting the balloons, I sent you just before Christmas, contained in copies of my letters to Sir Joseph Banks. There is no secret in the affair, and I make no doubt that a person coming from you would easily obtain a sight of the different balloons of Montgolfier and Charles, with all the instructions wanted; and, if you undertake to make one, I think it extremely proper and necessary to send an ingenious man here for that purpose; otherwise, for want of attention to some particular circumstance, or of not being acquainted with it, the experiment might miscarry, which, in an affair of so much public expectation, would have bad consequences, draw upon you a great deal of censure, and affect your reputation. It is a serious thing to draw out from their affairs all the inhabitants of a great city and its environs, and a disappointment makes them angry. At Bordeaux lately a person pretended to send up a balloon, and had received money from many people, but not being able to make it rise, the populace were so exasperated that they pulled down his house, and had like to have killed him.

It appears, as you observe, to be a discovery of great importance, and what may possibly give a new turn to human affairs. Convincing sovereigns of the folly of wars may perhaps be one effect of it; since it will be impracticable for the most potent of them to guard his dominions. Five thousand balloons, capable

of raising two men each, could not cost more than five ships of the line; and where is the prince who can afford so to cover his country with troops for its defence, as that ten thousand men descending from the clouds might not in many places do an infinite deal of mischief, before a force could be brought together to repel them? It is a pity that any national jealousy should, as you imagine it may, have prevented the English from prosecuting the experiment, since they are such ingenious mechanicians, that in their hands it might have made a more rapid progress towards perfection, and all the utility it is capable of affording.

The balloon of Messrs. Charles and Robert was really filled with inflammable air. The quantity being great, it was expensive, and tedious filling, requiring two or three days and nights constant labor. It had a *soupape*, or valve, near the top, which they could open by pulling a string, and thereby let out some air when they had a mind to descend; and they discharged some of their ballast of sand when they would rise again. A great deal of air must have been let out when they landed, so that the loose part might envelope one of them; yet, the car being lightened by that one getting out of it, there was enough left to carry up the other rapidly. They had no fire with them. That is used only in M. Montgolfier's globe, which is open at bottom, and straw constantly burnt to keep it up. This kind is sooner and cheaper filled; but must be of much greater dimensions to carry up the same weight; since air rarefied by heat is only twice as light as common air, and inflammable air ten times lighter. M. Morveau, a famous chemist at Dijon, has discovered an inflammable air that will cost only a twenty-fifth part of the price of what is made by oil

of vitriol poured on iron filings. They say it is made from sea coal. Its comparative weight is not mentioned.

I am as ever, my dear friend,

Yours most affectionately,

B. FRANKLIN.

TO BENJAMIN VAUGHAN.

Fire supposed to exist in the State of a Subtile Fluid.

Passy, 29 April, 1784.

MY DEAR FRIEND,

I received your kind letters of the 16th and 20th instant. I thank you for your philosophical news. We have none here. I see your philosophers are in the way of finding out at last what fire is. I have long been of opinion, that it exists everywhere in the state of a subtile fluid; that too much of that fluid in our flesh gives us the sensation we call heat; too little, cold; its vibrations, light. That all solid or fluid substances, which are inflammable, have been composed of it; their dissolution in returning to their original fluid state, we call fire. This subtile fluid is attracted by plants and animals in their growth, and consolidated; is attracted by other substances, thermometers, &c. variously; has a particular affinity with water, and will quit many other bodies to attach itself to water, and go off with it in evaporation. Adieu.

Yours, most sincerely,

B. FRANKLIN.

*Meteorological Imaginations and Conjectures.**

Passy, May, 1784.

There seems to be a region high in the air over all countries, where it is always winter, where frost exists continually, since in the midst of summer, on the surface of the earth, ice falls often from above, in the form of hail.

Hailstones, of the great weight we sometimes find them, did not probably acquire their magnitude before they began to descend. The air, being eight hundred times rarer than water, is unable to support it but in the shape of vapor, a state in which its particles are separated. As soon as they are condensed by the cold of the upper region, so as to form a drop, that drop begins to fall. If it freezes into a grain of ice, that ice descends. In descending, both the drop of water and the grain of ice are augmented by particles of the vapor they pass through in falling, and which they condense by coldness, and attach to themselves.

It is possible, that, in summer, much of what is rain when it arrives at the surface of the earth, might have been snow when it began its descent; but, being thawed in passing through the warm air near the surface, it is changed from snow into rain.

How immensely cold must be the original particle of hail, which forms the centre of the future hailstone, since it is capable of communicating sufficient cold, if I may so speak, to freeze all the mass of vapor condensed round it, and form a lump of perhaps six or eight ounces in weight!

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* First printed in the *Memoirs of the Literary and Philosophical Society of Manchester*, Vol. II. p. 357. It was communicated to the Society by Dr. Percival, and read December 22d, 1784.— EDITOR.

When, in summer time, the sun is high, and continues long every day above the horizon, his rays strike the earth more directly, and with longer continuance, than in the winter; hence the surface is more heated, and to a greater depth, by the effect of those rays.

When rain falls on the heated earth, and soaks down into it, it carries down with it a great part of the heat, which by that means descends still deeper.

The mass of earth, to the depth perhaps of thirty feet, being thus heated to a certain degree, continues to retain its heat for some time. Thus the first snows, that fall in the beginning of winter, seldom lie long on the surface, but are soon melted, and soon absorbed. After which, the winds, that blow over the country on which the snows had fallen, are not rendered so cold as they would have been, by those snows, if they had remained, and thus the approach of the severity of winter is retarded; and the extreme degree of its cold is not always at the time we might expect it, viz. when the sun is at its greatest distance, and the day shortest, but some time after that period, according to the English proverb, which says, "As the day lengthens, the cold strengthens;" the causes of refrigeration continuing to operate, while the sun returns too slowly, and his force continues too weak, to counteract them.

During several of the summer months of the year 1783, when the effects of the sun's rays to heat the earth in these northern regions should have been the greatest, there existed a constant fog over all Europe, and great part of North America. This fog was of a permanent nature; it was dry, and the rays of the sun seemed to have little effect towards dissipating it, as they easily do a moist fog, arising from water. They were indeed rendered so faint in passing through it,

that, when collected in the focus of a burning-glass, they would scarce kindle brown paper. Of course, their summer effect in heating the earth was exceedingly diminished.

Hence the surface was early frozen.

Hence the first snows remained on it unmelted, and received continual additions.

Hence perhaps the winter of 1783–4, was more severe than any that had happened for many years.

The cause of this universal fog is not yet ascertained. Whether it was adventitious to this earth, and merely a smoke proceeding from the consumption by fire of some of those great burning balls or globes which we happen to meet with in our rapid course round the sun, and which are sometimes seen to kindle and be destroyed in passing our atmosphere, and whose smoke might be attracted and retained by our earth; or whether it was the vast quantity of smoke, long continuing to issue during the summer from Hecla, in Iceland, and that other volcano which arose out of the sea near that island, which smoke might be spread by various winds, over the northern part of the world, is yet uncertain.

It seems however worth the inquiry, whether other hard winters, recorded in history, were preceded by similar permanent and widely extended summer fogs. Because, if found to be so, men might from such fogs conjecture the probability of a succeeding hard winter, and of the damage to be expected by the breaking up of frozen rivers in the spring; and take such measures as are possible and practicable, to secure themselves and effects from the mischiefs that attended the last.

Loose Thoughts on a Universal Fluid.

Passy, 25 June, 1784.

Universal space, as far as we know of it, seems to be filled with a subtile fluid, whose motion, or vibration, is called light.

This fluid may possibly be the same with that, which, being attracted by, and entering into other more solid matter, dilates the substance, by separating the constituent particles, and so rendering some solids fluid, and maintaining the fluidity of others; of which fluid when our bodies are totally deprived, they are said to be frozen; when they have a proper quantity, they are in health, and fit to perform all their functions; it is then called natural heat; when too much, it is called fever; and, when forced into the body in too great a quantity from without, it gives pain by separating and destroying the flesh, and is then called burning; and the fluid so entering and acting is called fire.

While organized bodies, animal or vegetable, are augmenting in growth, or are supplying their continual waste, is not this done by attracting and consolidating this fluid called fire, so as to form of it a part of their substance; and is it not a separation of the parts of such substance, which, dissolving its solid state, sets that subtile fluid at liberty, when it again makes its appearance as fire?

For the power of man relative to matter seems limited to the dividing it, or mixing the various kinds of it, or changing its form and appearance by different compositions of it; but does not extend to the making or creating of new matter, or annihilating the old. Thus, if fire be an original element, or kind of matter, its quantity is fixed and permanent in the world. We

cannot destroy any part of it, or make addition to it; we can only separate it from that which confines it, and so set it at liberty, as when we put wood in a situation to be burnt; or transfer it from one solid to another, as when we make lime by burning stone, a part of the fire dislodged from the wood being left in the stone. May not this fluid, when at liberty, be capable of penetrating and entering into all bodies organized or not, quitting easily in totality those not organized; and quitting easily in part those which are; the part assumed and fixed remaining till the body is dissolved?

Is it not this fluid which keeps asunder the particles of air, permitting them to approach, or separating them more, in proportion as its quantity is diminished or augmented? Is it not the greater gravity of the particles of air, which forces the particles of this fluid to mount with the matters to which it is attached, as smoke or vapor?

Does it not seem to have a great affinity with water, since it will quit a solid to unite with that fluid, and go off with it in vapor, leaving the solid cold to the touch, and the degree measurable by the thermometer?

The vapor rises attached to this fluid, but at a certain height they separate, and the vapor descends in rain, retaining but little of it, in snow or hail less. What becomes of that fluid? Does it rise above our atmosphere, and mix equally with the universal mass of the same kind? Or does a spherical stratum of it, denser, or less mixed with air, attracted by this globe, and repelled or pushed up only to a certain height from its surface, by the greater weight of air, remain there, surrounding the globe, and proceeding with it round the sun?

In such case, as there may be a continuity or communication of this fluid through the air quite down to

the earth, is it not by the vibrations given to it by the sun that light appears to us; and may it not be, that every one of the infinitely small vibrations, striking common matter with a certain force, enters its substance, is held there by attraction, and augmented by succeeding vibrations, till the matter has received as much as their force can drive into it?

Is it not thus, that the surface of this globe is continually heated by such repeated vibrations in the day, and cooled by the escape of the heat, when those vibrations are discontinued in the night, or intercepted and reflected by clouds?

Is it not thus that fire is amassed, and makes the greatest part of the substance of combustible bodies?

Perhaps, when this globe was first formed, and its original particles took their place at certain distances from the centre, in proportion to their greater or less gravity, the fluid fire, attracted towards that centre, might in great part be obliged, as lightest, to take place above the rest, and thus form the sphere of fire above supposed, which would afterwards be continually diminishing by the substance it afforded to organized bodies, and the quantity restored to it again by the burning or other separating of the parts of those bodies.

Is not the natural heat of animals thus produced, by separating in digestion the parts of food, and setting their fire at liberty?

Is it not this sphere of fire, which kindles the wandering globes that sometimes pass through it in our course round the sun, have their surface kindled by it, and burst when their included air is greatly rarefied by the heat on their burning surfaces?*

* This paper was read before the American Philosophical Society, June 20th, 1788, as a letter to David Rittenhouse, with the addition only of the following sentence, viz. "May it not have been from such considerations, that the ancient philosophers supposed a sphere of fire to exist above the air of our atmosphere?"—EDITOR.

TO JOHN INGENHOUSZ.*

Clock with three Wheels. — Gravitation of Bodies.

Passy, 29 April, 1785.

I do not know that my contrivance of a clock with three wheels only, which showed hours, minutes, and seconds, has ever been published. I have seen several of them here at Paris, that were made by Mr. Whitehurst, and sent over, I believe, by Mr. Magellan. You are welcome to do what you please with it. Mr. Whitehurst's invention is very simple, and should be very effectual, provided the foot of the rod and the situation of the clock are invariably fixed, so as never to be at a greater or less distance from one another, which may be by fixing both in a straight-grained piece of wood of about four feet long; wood not changing its dimensions the lengthwise of the grain, by any common degree of heat or cold. But this cannot be trusted to in the wood of a clock-case, because in sawing boards the grain is frequently crossed, and moisture and dryness will change their dimensions.

You are at liberty also to publish, if you think fit, the experiment of the globe floating between two liquors. I suppose you remember to have seen it on my chimney-piece. Though it is a matter of no utility. Something of the same nature has been done more than a hundred years since by another person, I forget who.

What I formerly mentioned to you of hanging a weight on a spiral spring, to discover if bodies gravitated differently to the earth during the conjunctions of the sun and moon, compared with other times, was this. We suppose, that, by the force of gravity in those luminaries, the water of the ocean, an immense weight, is

* For a part of this letter, on Electricity, see Vol. V. p. 480. — Editor.

elevated so as to form the tides; if that be so, might we not expect that an iron ball of a pound weight, suspended by a fine spiral spring, should, when the sun and moon are together both above it, be a little attracted upwards or rendered lighter, so as to be drawn up a little by the spring on which it depends, and the contrary when they are both below it. [See Plate XI. Fig. 2.] The quantity, though very small, might perhaps be rendered visible by a contrivance like the above. It is not difficult to make this experiment, but I have never made it. With regard to the tides, I doubt the opinion of there being but two high waters and two low waters existing at the same time on the globe. I rather think there are many, and those at the distance of about one hundred leagues from each other. The tides found in the River Amazons seem to favor this opinion. Observations hereafter in the isles of the Pacific Ocean may confirm or refute it.

If I were in a situation where I could be a little more master of my time, I would, as you desire, write my ideas on the subject of chimneys. They might, I think, be useful; for by what I see everywhere the subject seems too little understood, which occasions much inconvenience and fruitless expense. But, besides being harassed by too much business, I am exposed to numberless visits, some of kindness and civility, many of mere idle curiosity, from strangers of America and of different parts of Europe, as well as the inhabitants of the provinces who come to Paris. These devour my hours, and break my attention, and at night I often find myself fatigued without having done any thing. Celebrity may for a while flatters one's vanity, but its effects are troublesome. I have begun to write two or three things, which I wish to finish before I die; but I sometimes doubt the possibility.

B. FRANKLIN.

TO DAVID LE ROY, AT PARIS.*

Improvements in Navigation. — Observations on the Sails and Cables of Vessels; the Models for their Construction; Means of preserving them from Accidents at Sea; Methods of giving Motion to Boats. — Gulf Stream. — Precautions to those who are about to take a Voyage at Sea. — General Reflections on Navigation.

READ AT A MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY, DECEMBER 2D, 1785.

At sea, on board the London Packet,
Captain Truxton, August, 1785.

SIR,

Your learned writings on the navigation of the ancients, which contain a great deal of curious information, and your very ingenious contrivances for improving the modern sails (*voilure*), of which I saw with great pleasure a successful trial on the river Seine, have induced me to submit to your consideration and judgment, some thoughts I have had on the latter subject.

Those mathematicians, who have endeavoured to improve the swiftness of vessels by calculating to find the form of least resistance, seem to have considered a ship

* This letter was translated into French, and published at Paris in the year 1787, entitled, "*Lettre de Monsieur Benjamin Franklin à Monsieur David Le Roy, Membre de Plusieurs Académies, &c.*" The following note is prefixed by the French editor. — "Cette lettre a été lue à la Société Philosophique Américaine de Philadelphia, le 2 Décembre, 1785. Elle est imprimée dans les Mémoires de cette Société. On lit dans le titre, *à M. Alphonse Le Roy*. Comme cet Académicien ne se nomme pas Alphonse, nous y avons substitué l'un de ses noms de baptême. Il est de l'Académie des Belles-Lettres, de celle de Marine, de la Société des Antiquaires de Londres, de la Société Philosophique Américaine, &c." Hitherto the letter has been printed as addressed to *Alphonsus Le Roy*. The address is now changed to *David Le Roy*, in conformity with the French edition. — EDITOR.

as a body moving through one fluid only, the water; and to have given little attention to the circumstance of her moving through another fluid, the air. It is true, that, when a vessel sails right before the wind, this circumstance is of no importance, because the wind goes with her; but, in every deviation from that course, the resistance of the air is something, and becomes greater in proportion as that deviation increases. I wave at present the consideration of those different degrees of resistance given by the air to that part of the hull which is above water, and confine myself to that given to the sails; for their motion through the air is resisted by the air, as the motion of the hull through the water is resisted by the water, though with less force as the air is a lighter fluid. And, to simplify the discussion as much as possible, I would state one situation only, to wit, that of the wind upon the beam, the ship's course being directly across the wind; and I would suppose the sail set in an angle of forty-five degrees with the keel, as in the following figure; wherein (Plate XII. Fig. 1,) *A B* represents the body of the vessel, *C D* the position of the sail, *E E E* the direction of the wind, *M M* the line of motion. In observing this figure it will appear, that so much of the body of the vessel as is immersed in the water must, to go forward, remove out of its way what water it meets with between the pricked lines *F F*. And the sail, to go forward, must move out of its way all the air its whole dimension meets with between the pricked lines *C G* and *D G*. Thus both the fluids give resistance to the motion, each in proportion to the quantity of matter contained in the dimension to be removed. And, though the air is vastly lighter than the water, and therefore more easily removed, yet, the dimension being much greater, its effect is very considerable.

It is true, that, in the case stated, the resistance given

by the air between those lines to the motion of the sail is not apparent to the eye, because the greater force of the wind, which strikes it in the direction *E E E*, overpowers its effect, and keeps the sail full in the curve *a, a, a, a, a.* But, suppose the wind to cease, and the vessel in a calm to be impelled with the same swiftness by oars, the sail would then appear filled in the contrary curve *b, b, b, b, b,* when prudent men would immediately perceive, that the air resisted its motion, and would order it to be taken in.

Is there any possible means of diminishing this resistance, while the same quantity of sail is exposed to the action of the wind, and therefore the same force obtained from it? I think there is, and that it may be done by dividing the sail into a number of parts, and placing those parts in a line one behind the other; thus instead of one sail extending from *C* to *D*, figure 2, if four sails, containing together the same quantity of canvass, were placed as in figure 3, each having one quarter of the dimensions of the great sail, and exposing a quarter of its surface to the wind, would give a quarter of the force; so that the whole force obtained from the wind would be the same, while the resistance from the air would be nearly reduced to the space between the pricked lines *a b* and *c d*, before the foremost sail.

It may perhaps be doubted whether the resistance from the air would be so diminished; since possibly each of the following small sails having also air before it, which must be removed, the resistance on the whole would be the same.

This is then a matter to be determined by experiment. I will mention one, that I many years since made with success for another purpose; and I will propose another small one easily made. If that too succeeds, I should think it worth while to make a larger, though at some expense, on a river boat; and perhaps

time, and the improvements experience will afford, may make it applicable with advantage to larger vessels.

Having near my kitchen chimney a round hole of eight inches diameter, through which was a constant steady current of air, increasing or diminishing only as the fire increased or diminished, I contrived to place my jack so as to receive that current; and, taking off the fliers, I fixed in their stead on the same pivot a round tin plate of nearly the same diameter with the hole; and having cut it in radial lines almost to the centre, so as to have six equal vanes, I gave to each of them the obliquity of forty-five degrees. They moved round, without the weight, by the impression only of the current of air, but too slowly for the purpose of roasting. I suspected that the air struck by the back of each vane might possibly by its resistance retard the motion; and to try this, I cut each of them into two, and I placed the twelve, each having the same obliquity, in a line behind each other, when I perceived a great augmentation in its velocity, which encouraged me to divide them once more, and, continuing the same obliquity, I placed the twenty-four behind each other in a line, when the force of the wind being the same, and the surface of vane the same, they moved round with much greater rapidity, and perfectly answered my purpose.

The second experiment that I propose, is, to take two playing cards, of the same dimensions, and cut one of them transversely into eight equal pieces; then with a needle string them upon two threads, one near each end, and place them so upon the threads that, when hung up, they may be one exactly over the other, at a distance equal to their breadth, each in a horizontal position; and let a small weight, such as a bird-shot, be hung under them, to make them fall in a straight line when let loose. Suspend also the whole card by threads

from its four corners, and hang to it an equal weight, so as to draw it downwards when let fall, its whole breadth pressing against the air. Let those two bodies be attached, one of them to one end of a thread a yard long, the other to the other end. Extend a twine under the ceiling of a room, and put through it at thirty inches distance two pins bent in the form of fish-hooks. On these two hooks hang the two bodies, the thread that connects them extending parallel to the twine, which thread being cut, they must begin to fall at the same instant. If they take equal time in falling to the floor, it is a proof that the resistance of the air is in both cases equal. If the whole card requires a longer time, it shows that the sum of the resistances to the pieces of the cut card is not equal to the resistance of the whole one.*

This principle so far confirmed, I would proceed to make a larger experiment, with a shallop, which I would rig in this manner. (Plate XII. Fig. 4.) *A B* is a long boom, from which are hoisted seven jibs, *a*, *b*, *c*, *d*, *e*, *f*, *g*, each a seventh part of the whole dimensions, and as much more as will fill the whole space when set in an angle of forty-five degrees, so that they may lap when going before the wind, and hold more wind when going large. Thus rigged, when going right before the wind, the boom should be brought at right angles with the keel, by means of the sheet ropes *C D*, and all the sails hauled flat to the boom.

These positions of boom and sails to be varied as the wind quarters. But when the wind is on the beam, or when you would turn to windward, the boom is to be hauled right fore and aft, and the sails trimmed according as the wind is more or less against your course.

* The motion of the vessel made it inconvenient to try this simple experiment at sea, when the proposal of it was written. But it has been tried since we came on shore, and succeeded as the other.

It seems to me, that the management of a shallop so rigged would be very easy, the sails being run up and down separately, so that more or less sail may be made at pleasure; and I imagine, that there being full as much sail exposed to the force of the wind which impels the vessel in its course, as if the whole were in one piece, and the resistance of the dead air against the foreside of the sail being diminished, the advantage of swiftness would be very considerable; besides that the vessel would lie nearer the wind.

Since we are on the subject of improvements in navigation, permit me to detain you a little longer with a small relative observation. Being, in one of my voyages, with ten merchant ships under convoy of a frigate at anchor in Torbay, waiting for a wind to go to the westward, it came fair, but brought in with it a considerable swell. A signal was given for weighing, and we put to sea all together; but three of the ships left their anchors, their cables parting just as the anchors came a-peak. Our cable held, and we got up our anchor; but the shocks the ship felt before the anchor got loose from the ground, made me reflect on what might possibly have caused the breaking of the other cables; and I imagined it might be the short bending of the cable just without the hawse-hole, from a horizontal to an almost vertical position, and the sudden violent jerk it receives by the rising of the head of the ship on the swell of a wave while in that position. For example, suppose a vessel hove up so as to have her head nearly over her anchor, which still keeps its hold, perhaps in a tough bottom; if it were calm, the cable still out would form nearly a perpendicular line, measuring the distance between the hawse-hole and the anchor; but if there is a swell, her head in the trough of the sea will fall below the level, and when lifted on the wave

will be much above it. In the first case the cable will hang loose and bend perhaps as in figure 5. In the second case, figure 6, the cable will be drawn straight with a jerk, must sustain the whole force of the rising ship, and must either loosen the anchor, resist the rising force of the ship, or break. But why does it break at the hawse-hole?

Let us suppose it a cable of three inches diameter, and represented by figure 7. If this cable is to be bent round the corner *A*, it is evident that either the part of the triangle contained between the letters *a*, *b*, *c*, must stretch considerably, and those most that are nearest the surface; or that the parts between *d*, *e*, *f*, must be compressed; or both, which most probably happens. In this case, the lower half of the thickness affords no strength against the jerk, it not being strained, the upper half bears the whole, and the yarns near the upper surface being first and most strained, break first, and the next yarns follow; for in this bent situation they cannot bear the strain all together, and each contribute its strength to the whole, as they do when the cable is strained in a straight line.

To remedy this, methinks it would be well to have a kind of large pulley wheel, fixed in the hawse-hole, suppose of two feet diameter, over which the cable might pass; and, being there bent gradually to the round of the wheel, would thereby be more equally strained, and better able to bear the jerk, which may save the anchor, and by that means in the course of the voyage may happen to save the ship.

One maritime observation more shall finish this letter. I have been a reader of newspapers now near seventy years, and I think few years pass without an account of some vessel met with at sea, with no living soul on board, and so many feet of water in her hold, which

vessel has nevertheless been saved and brought into port; and when not met with at sea, such forsaken vessels have often come ashore on some coast. The crews, who have taken to their boats and thus abandoned such vessels, are sometimes met with and taken up at sea by other ships, sometimes reach a coast, and are sometimes never heard of. Those that give an account of quitting their vessels generally say, that she sprung a leak, that they pumped for some time, that the water continued to rise upon them, and that, despairing to save her, they had quitted her, lest they should go down with her. It seems by the event that this fear was not always well founded, and I have endeavoured to guess at the reason of the people's too hasty discouragement.

When a vessel springs a leak near her bottom, the water enters with all the force given by the weight of the column of water without, which force is in proportion to the difference of level between the water without and that within. It enters therefore with more force at first and in greater quantity, than it can afterwards when the water within is higher. The bottom of the vessel too is narrower, so that the same quantity of water coming into that narrow part, rises faster than when the space for it to flow in is larger. This helps to terrify. But, as the quantity entering is less and less as the surfaces without and within become more nearly equal in height, the pumps, that could not keep the water from rising at first, might afterwards be able to prevent its rising higher, and the people might have remained on board in safety, without hazarding themselves in an open boat on the wide ocean. (Fig. 8.)

Besides the greater equality in the height of the two surfaces, there may sometimes be other causes that retard the farther sinking of a leaky vessel. The rising water within may arrive at quantities of light wooden

work, empty chests, and particularly empty water-casks, which if fixed so as not to float themselves may help to sustain her. Many bodies which compose a ship's cargo may be specifically lighter than water; all these when out of water are an additional weight to that of the ship, and she is in proportion pressed deeper into the water; but, as soon as these bodies are immersed, they weigh no longer on the ship, but on the contrary, if fixed, they help to support her, in proportion as they are specifically lighter than the water. And it should be remembered, that the largest body of a ship may be so balanced in the water, that an ounce less or more of weight may leave her at the surface or sink her to the bottom. There are also certain heavy cargoes, that, when the water gets at them, are continually dissolving, and thereby lightening the vessel, such as salt and sugar. And as to water-casks, mentioned above, since the quantity of them must be great in ships of war, where the number of men consume a great deal of water every day, if it had been made a constant rule to bung them up as fast as they were emptied, and to dispose the empty casks in proper situations, I am persuaded that many ships which have been sunk in engagements, or have gone down afterwards, might with the unhappy people have been saved; as well as many of those which in the last war foundered, and were never heard of. While on this topic of sinking, one cannot help recollecting the well-known practice of the Chinese, to divide the hold of a great ship into a number of separate chambers by partitions tight caulked (of which you gave a model in your boat upon the Seine), so that, if a leak should spring in one of them, the others are not affected by it; and, though that chamber should fill to a level with the sea, it would not be sufficient to sink the vessel. We have not imitated

this practice. Some little disadvantage it might occasion in the stowage, is perhaps one reason, though that I think might be more than compensated by an abatement in the insurance that would be reasonable, and by a higher price taken of passengers, who would rather prefer going in such a vessel. But our seafaring people are brave, despise danger, and reject such precautions of safety, being cowards only in one sense, that of *fearing* to be *thought afraid.*

I promised to finish my letter with the last observation, but the garrulity of the old man has got hold of me, and, as I may never have another occasion of writing on this subject, I think I may as well now, once for all, empty my nautical budget, and give you all the thoughts that have in my various long voyages occurred to me relating to navigation. I am sure, that in you they will meet with a candid judge, who will excuse my mistakes on account of my good intention.

There are six accidents, that may occasion the loss of ships at sea. We have considered one of them, that of foundering by a leak. The other five are, 1. Oversetting by sudden flaws of wind, or by carrying sail beyond the bearing. 2. Fire by accident or carelessness. 3. A heavy stroke of lightning, making a breach in the ship, or firing the powder. 4. Meeting and shocking with other ships in the night. 5. Meeting in the night with islands of ice.

To that of oversetting, privateers in their first cruise have, as far as has fallen within my knowledge or information, been more subject than any other kind of vessels. The double desire of being able to overtake a weaker flying enemy, or to escape when pursued by a stronger, has induced the owners to overmast their cruisers, and to spread too much canvass; and the great number of men, many of them not seamen, who, being

upon deck when a ship heels suddenly, are huddled down to leeward, and increase by their weight the effect of the wind. This therefore should be more attended to and guarded against, especially as the advantage of lofty masts is problematical. For the upper sails have greater power to lay a vessel more on her side, which is not the most advantageous position for going swiftly through the water. And hence it is, that vessels, which have lost their lofty masts, and been able to make little more sail afterwards than permitted the ship to sail upon an even keel, have made so much way, even under jury masts, as to surprise the mariners themselves. But there is, besides, something in the modern form of our ships, that seems as if calculated expressly to allow their oversetting more easily. The sides of a ship, instead of spreading out as they formerly did in the upper works, are of late years turned in, so as to make the body nearly round, and more resembling a cask. I do not know what the advantages of this construction are, except that such ships are not easily boarded. To me it seems a contrivance to have less room in a ship at nearly the same expense. For it is evident, that the same timber and plank consumed in raising the sides from *a* to *b*, and from *d* to *c*, would have raised them from *a* to *e*, and from *d* to *f*, fig. 9. In this form all the spaces between *e*, *a*, *b*, and *c*, *d*, *f*, would have been gained, the deck would have been larger, the men would have had more room to act, and not have stood so thick in the way of the enemy's shot; and the vessel, the more she was laid down on her side, the more bearing she would meet with, and more effectual to support her, as being farther from the centre. Whereas, in the present form, her ballast makes the chief part of her bearing, without which she would turn in the sea almost as easily as a barrel. More

ballast by this means becomes necessary, and that, sinking a vessel deeper in the water, occasions more resistance to her going through it. The Bermudian sloops still keep with advantage to the old spreading form.

The islanders in the great Pacific ocean, though they have no large ships, are the most expert boat-sailors in the world, navigating that sea safely with their prôas, which they prevent oversetting by various means. Their sailing proas for this purpose have outriggers generally to windward, above the water, on which, one or more men are placed, to move occasionally further from or nearer to the vessel as the wind freshens or slackens. But some have their outriggers to leeward, which, resting on the water, support the boat so as to keep her upright when pressed down by the wind. Their boats, moved by oars or rather by paddles, are, for long voyages, fixed two together by cross bars of wood that keep them at some distance from each other, and so render their oversetting next to impossible. How far this may be practicable in larger vessels, we have not yet sufficient experience. I know of but one trial made in Europe, which was about one hundred years since, by Sir William Petty. He built a double vessel, to serve as a packet-boat between England and Ireland. Her model still exists in the museum of the Royal Society, where I have seen it. By the accounts we have of her, she answered well the purpose of her construction, making several voyages; and, though wrecked at last by a storm, the misfortune did not appear owing to her particular construction, since many other vessels of the common form were wrecked at the same time. The advantage of such a vessel is, that she needs no ballast, therefore swims either lighter, or will carry more goods; and that passengers are not so much incommoded by her rolling; to which may be

added, that if she is to defend herself by her cannon, they will probably have more effect, being kept more generally in a horizontal position, than those in common vessels. I think, however, that it would be an improvement of that model, to make the sides which are opposed to each other perfectly parallel, though the other sides are formed as in common, thus, figure 10.

The building of a double ship would indeed be more expensive in proportion to her burthen; and that perhaps is sufficien to discourage the method.

The accident of fire is generally well guarded against by the prudent captain's strict orders against smoking between decks, or carrying a candle there out of a lantern. But there is one dangerous practice which frequent terrible accidents have not yet been sufficient to abolish; that of carrying store spirits to sea in casks. Two large ships, the *Serapis* and the *Duke of Athol*, one an East Indiaman, the other a frigate, have been burnt within these two last years, and many lives miserably destroyed, by drawing spirits out of a cask near a candle. It is high time to make it a general rule, that all the ship's store of spirits should be carried in bottles.

The misfortune by a stroke of lightning I have in my former writings endeavoured to show a method of guarding against, by a chain and pointed rod, extending, when run up, from above the top of the mast to the sea. These instruments are now made and sold at a reasonable price by Nairne & Co. in London, and there are several instances of success attending the use of them. They are kept in a box, and may be run up and fixed in about five minutes, on the apparent approach of a thunder-gust.

Of the meeting and shocking with other ships in the night, I have known two instances in voyages between

London and America. In one, both ships arrived, though much damaged, each reporting their belief that the other must have gone to the bottom. In the other, only one got to port; the other was never afterwards heard of. These instances happened many years ago, when the commerce between Europe and America was not a tenth part of what it is at present, ships of course thinner scattered, and the chance of meeting proportionably less. It has long been the practice to keep a *look-out before* in the channel, but at sea it has been neglected. If it is not at present thought worth while to take that precaution, it will in time become of more consequence; since the number of ships at sea is continually augmenting. A drum frequently beat, or a bell rung in a dark night, might help to prevent such accidents.

Islands of ice are frequently seen off the banks of Newfoundland by ships going between North America and Europe. In the day time they are easily avoided, unless in a very thick fog. I remember two instances of ships running against them in the night. The first lost her bowsprit, but received little other damage. The other struck where the warmth of the sea had wasted the ice next to it, and a part hung over above. This perhaps saved her, for she was under great way; but the upper part of the cliff, taking her foretopmast, broke the shock, though it carried away the mast. She disengaged herself with some difficulty, and got safe into port; but the accident shows the possibility of other ships being wrecked and sunk by striking those vast masses of ice, of which I have seen one that we judged to be seventy feet high above the water, consequently eight times as much under water; and it is another reason for keeping a good *look-out before*, though far from any coast that may threaten danger.

It is remarkable, that the people we consider as

savages, have improved the art of sailing and rowing boats in several points beyond what we can pretend to.

We have no sailing boats equal to the flying proas of the South Seas, no rowing or paddling boat equal to that of the Greenlanders, for swiftness and safety. The birch canoes of the North American Indians have also some advantageous properties. They are so light that two men may carry one of them over land, which is capable of carrying a dozen upon the water; and in heeling they are not so subject to take in water as our boats, the sides of which are lowest in the middle where it is most likely to enter, this being highest in that part, as in figure 11.

The Chinese are an enlightened people, the most anciently civilized of any existing, and their arts are ancient, a presumption in their favor; their method of rowing their boats differs from ours, the oars being worked either two a-stern, as we scull, or on the sides with the same kind of motion, being hung parallel to the keel on a rail, and always acting in the water, not perpendicular to the side, as ours are, nor lifted out at every stroke, which is a loss of time, and the boat in the interval loses motion. They see our manner, and we theirs, but neither are disposed to learn of or copy the other.

To the several means of moving boats mentioned above, may be added the singular one lately exhibited at Javelle, on the Seine below Paris, where a clumsy boat was moved across that river in three minutes by rowing, not in the water, but in the air, that is, by whirling round a set of windmill vanes fixed to a horizontal axis, parallel to the keel, and placed at the head of the boat. The axis was bent into an elbow at the end, by the help of which it was turned by one man at a time. I saw the operation at a distance. The four

vanes appeared to be about five feet long, and perhaps two and a half wide. The weather was calm. The labor appeared to be great for one man, as the two several times relieved each other. But the action upon the air by the oblique surfaces of the vanes must have been considerable, as the motion of the boat appeared tolerably quick going and returning; and she returned to the same place from whence she first set out, notwithstanding the current. This machine is since applied to the moving of air-balloons; an instrument similar may be contrived to move a boat by turning under water.

Several mechanical projectors have at different times proposed to give motion to boats, and even to ships, by means of circular rowing, or paddles placed on the circumference of wheels to be turned constantly on each side of the vessel; but this method, though frequently tried, has never been found so effectual as to encourage a continuance of the practice. I do not know that the reason has hitherto been given. Perhaps it may be this, that great part of the force employed contributes little to the motion. For instance, (fig. 12) of the four paddles *a*, *b*, *c*, *d*, all under water, and turning to move a boat from *X* to *Y*, *c* has the most power, *b* nearly though not quite as much, their motion being nearly horizontal; but the force employed in moving *a*, is consumed in pressing almost downright upon the water till it comes to the place of *b*; and the force employed in moving *d* is consumed in lifting the water till *d* arrives at the surface; by which means much of the labor is lost. It is true, that by placing the wheels higher out of the water, this waste labor will be diminished in a calm, but where a sea runs, the wheels must unavoidably be often dipped deep in the waves, and the turning of them thereby rendered very laborious to little purpose.

Among the various means of giving motion to a boat, that of M. Bernoulli appears one of the most singular, which was to have fixed in the boat a tube in the form of an L, the upright part to have a funnel-kind of opening at top, convenient for filling the tube with water; which, descending and passing through the lower horizontal part, and issuing in the middle of the stern, but under the surface of the river, should push the boat forward. There is no doubt that the force of the descending water would have a considerable effect, greater in proportion to the height from which it descended; but then it is to be considered, that every bucket-full pumped or dipped up into the boat, from its side or through its bottom, must have its *vis inertiæ* overcome so as to receive the motion of the boat, before it can come to give motion by its descent; and that will be a deduction from the moving power. To remedy this, I would propose the addition of another such L pipe, and that they should stand back to back in the boat thus, figure 13; the forward one being worked as a pump, and sucking in the water at the head of the boat, would draw it forward while pushed in the same direction by the force at the stern. And after all it should be calculated whether the labor of pumping would be less than that of rowing. A fire-engine might possibly in some cases be applied in this operation with advantage.

Perhaps this labor of raising water might be spared, and the whole force of a man applied to the moving of a boat by the use of air instead of water. Suppose the boat constructed in this form, figure 14. *A*, a tube round or square of two feet diameter, in which a piston may move up and down. The piston to have valves in it, opening inwards to admit air when the piston rises; and shutting, when it is forced down by means of the

lever *B* turning on the centre *C*. The tube to have a valve *D*, to open when the piston is forced down, and let the air pass out at *E*, which striking forcibly against the water abaft must push the boat forward. If there is added an air-vessel *F* properly valved and placed, the force would continue to act while a fresh stroke is taken with the lever. The boatman might stand with his back to the stern, and putting his hands behind him, work the motion by taking hold of the cross bar at *B*, while another should steer; or, if he had two such pumps, one on each side of the stern, with a lever for each hand, he might steer himself by working occasionally more or harder with either hand, as watermen now do with a pair of sculls. There is no position in which the body of a man can exert more strength than in pulling right upwards.

To obtain more swiftness, greasing the bottom of a vessel is sometimes used, and with good effect. I do not know that any writer has hitherto attempted to explain this. At first sight, one would imagine, that, though the friction of a hard body, sliding on another hard body, and the resistance occasioned by that friction, might be diminished by putting grease between them, yet that a body sliding on a fluid, such as water, should have no need of, nor receive, any advantage from such greasing. But the fact is not disputed. And the reason perhaps may be this. The particles of water have a mutual attraction, called the attraction of adhesion. Water also adheres to wood, and to many other substances, but not to grease; on the contrary they have a mutual repulsion, so that it is a question whether when oil is poured on water, they ever actually touch each other; for a drop of oil upon water, instead of sticking to the spot where it falls, as it would if it fell on a looking-glass, spreads instantly to an immense distance in a film extremely thin, which it could not easily

do if it touched and rubbed or adhered even in a small degree to the surface of the water. Now the adhesive force of water to itself, and to other substances, may be estimated from the weight of it necessary to separate a drop, which adheres, while growing, till it has weight enough to force the separation and break the drop off. Let us suppose the drop to be the size of a pea; then there will be as many of these adhesions as there are drops of that size touching the bottom of a vessel, and these must be broken by the moving power, every step of her motion that amounts to a drop's breadth; and there being no such adhesions to break between the water and a greased bottom, may occasion the difference.

So much respecting the motion of vessels. But we have sometimes occasion to stop their motion; and, if a bottom is near enough, we can cast anchor. Where there are no soundings, we have as yet no means to prevent driving in a storm, but by lying-to, which still permits driving at the rate of about two miles an hour; so that in a storm continuing fifty hours, which is not an uncommon case, the ship may drive one hundred miles out of her course; and should she in that distance meet with a lee shore, she may be lost.

To prevent this driving to leeward in deep water, a swimming anchor is wanting, which ought to have these properties.

1. It should have a surface so large as, being at the end of a hawser in the water, and placed perpendicularly, should hold so much of it, as to bring the ship's head to the wind, in which situation the wind has least power to drive her.

2. It should be able by its resistance to prevent the ship's receiving way.

3. It should be capable of being situated below the heave of the sea, but not below the undertow.

4. It should not take up much room in the ship.

5. It should be easily thrown out, and put into its proper situation.

6. It should be easy to take in again, and stow away.

An ingenious old mariner, whom I formerly knew, proposed, as a swimming anchor for a large ship, to have a stem of wood twenty-five feet long and four inches square, with four boards of eighteen, sixteen, fourteen, and twelve feet long, and one foot wide, the boards to have their substance thickened several inches in the middle by additional wood, and to have each a four inch square hole through its middle, to permit its being slipped on occasionally upon the stem, and at right angles with it; where, all being placed and fixed at four feet distance from each other, it would have the appearance of the old mathematical instrument called a forestaff. This thrown into the sea, and held by a hawser veered out to some length, he conceived would bring a vessel up, and prevent her driving, and when taken in might be stowed away by separating the boards from the stem; Figure 15. Probably such a swimming anchor would have some good effect; but it is subject to this objection, that, lying on the surface of the sea, it is liable to be hove forward by every wave, and thereby give so much leave for the ship to drive.

Two machines for this purpose have occurred to me, which, though not so simple as the above, I imagine would be more effectual, and more easily manageable. I will endeavour to describe them, that they may be submitted to your judgment, whether either would be serviceable; and, if they would, to which we should give the preference.

The first is to be formed, and to be used in the water on almost the same principles with those of a paper kite used in the air. Only, as the paper kite rises in

the air, this is to descend in the water. Its dimensions will be different for ships of different size.

To make one of suppose fifteen feet high; take a small spar of that length for the backbone, *A B*, figure 16, a smaller of half that length *C D*, for the cross piece. Let these be united by a bolt at *E*, yet so as that by turning on the bolt they may be laid parallel to each other. Then make a sail of strong canvass, in the shape of figure 17. To form this, without waste of sailcloth, sew together pieces of the proper length, and for half the breadth, as in figure 18, then cut the whole in the diagonal lines, *a*, *b*, *c*, and turn the piece *F* so as to place its broad part opposite to that of the piece *G*, and the piece *H* in like manner opposite to *I*, which when all sewed together will appear as in figure 17. This sail is to be extended on the cross of figure 16, the top and bottom points well secured to the ends of the long spar; the two side points *d*, *e*, fastened to the ends of two cords, which, coming from the angle of the loop (which must be similar to the loop of a kite), pass through two rings at the ends of the short spar, so as that on pulling upon the loop the sail will be drawn to its extent. The whole may, when aboard, be furled up, as in figure 19, having a rope from its broad end, to which is tied a bag of ballast for keeping that end downwards when in the water, and at the other end another rope with an empty keg at its end to float on the surface; this rope long enough to permit the kite's descending into the undertow, or if you please lower into still water. It should be held by a hawser. To get it home easily, a small loose rope may be veered out with it, fixed to the keg. Hauling on that rope will bring the kite home with small force, the resistance being small, as it will then come endways.

It seems probable that such a kite at the end of a

long hawser would keep a ship with her head to the wind, and, resisting every tug, would prevent her driving so fast as when her side is exposed to it, and nothing to hold her back. If only half the driving is prevented, so as that she moves but fifty miles instead of the hundred during a storm, it may be some advantage, both in holding so much distance as is saved, and in keeping from a lee-shore. If single canvass should not be found strong enough to bear the tug without splitting, it may be doubled, or strengthened by a netting behind it, represented by figure 20.

The other machine for the same purpose is to be made more in the form of an umbrella, as represented figure 21. The stem of the umbrella, a square spar of proper length, with four movable arms, of which two are represented *C, C*, figure 22. These arms to be fixed in four joint cleats, as *D, D*, &c., one on each side of the spar, but so as that the four arms may open by turning on a pin in the joint. When open, they form a cross, on which a four-square canvass sail is to be extended, its corners fastened to the ends of the four arms. Those ends are also to be stayed by ropes fastened to the stem or spar, so as to keep them short of being at right angles with it; and to the end of one of the arms should be hung the small bag of ballast, and to the end of the opposite arm the empty keg. This, on being thrown into the sea, would immediately open; and when it had performed its function, and the storm over, a small rope from its other end being pulled on, would turn it, close it, and draw it easily home to the ship. This machine seems more simple in its operation, and more easily manageable than the first, and perhaps may be as effectual.*

* Captain Truxtun, on board whose ship this was written, has executed this proposed machine; he has given six arms to the umbrella, they are

Vessels are sometimes retarded, and sometimes forwarded in their voyages, by currents at sea, which are often not perceived. About the year 1769 or 1670, there was an application made by the Board of Customs at Boston, to the Lords of the Treasury in London, complaining that the packets between Falmouth and New York were generally a fortnight longer in their passages, than merchant ships from London to Rhode Island, and proposing that for the future they should be ordered to Rhode Island instead of New York. Being then concerned in the management of the American post-office, I happened to be consulted on the occasion; and it appearing strange to me, that there should be such a difference between two places scarce a day's run asunder, especially when the merchant ships are generally deeper laden, and more weakly manned than the packets, and had from London the whole length of the river and channel to run before they left the land of England, while the packets had only to go from Falmouth, I could not but think the fact misunderstood or misrepresented. There happened then to be in London a Nantucket sea captain of my acquaintance, to whom I communicated the affair. He told me he believed the fact might be true; but the difference was owing to this, that the Rhode Island captains were acquainted with the Gulf Stream, which those of the English packets were not. "We are well acquainted with that stream," says he, "because in our pursuit of whales, which keep near the sides of it, but are not to be met with in it, we run down along the sides, and frequently cross it to change our side; and in crossing it have sometimes met and spoke with those packets, who were in the middle of it, and stemming it. We

joined to the stem by iron hinges, and the canvass is double. He has taken it with him to China. February, 1786.

have informed them that they were stemming a current, that was against them to the value of three miles an hour; and advised them to cross it and get out of it; but they were too wise to be counselled by simple American fishermen. When the winds are but light," he added, "they are carried back by the current more than they are forwarded by the wind; and, if the wind be good, the subtraction of seventy miles a day from their course is of some importance." I then observed it was a pity no notice was taken of this current upon the charts, and requested him to mark it out for me, which he readily complied with, adding directions for avoiding it in sailing from Europe to North America. I procured it to be engraved by order from the general post-office, on the old chart of the Atlantic, at Mount and Page's, Tower Hill; and copies were sent down to Falmouth for the captains of the packets, who slighted it however; but it is since printed in France, of which edition I hereto annex a copy. [See Plate XIII.]

This stream is probably generated by the great accumulation of water on the eastern coast of America between the tropics, by the trade winds which constantly blow there. It is known, that a large piece of water ten miles broad and generally only three feet deep, has by a strong wind had its waters driven to one side and sustained so as to become six feet deep, while the windward side was laid dry. This may give some idea of the quantity heaped up on the American coast, and the reason of its running down in a strong current through the islands into the bay of Mexico, and from thence issuing through the Gulf of Florida, and proceeding along the coast to the banks of Newfoundland, where it turns off towards and runs down through the Western Islands. Having since crossed this stream several times in passing between America and Europe,

I have been attentive to sundry circumstances relating to it, by which to know when one is in it; and besides the gulf weed with which it is interspersed, I find, that it is always warmer than the sea on each side of it, and that it does not sparkle in the night. I annex hereto the observations made with the thermometer in two voyages, and possibly may add a third. It will appear from them, that the thermometer may be a useful instrument to a navigator, since currents coming from the northward into southern seas will probably be found colder than the water of those seas, as the currents from southern seas into northern are found warmer. And it is not to be wondered, that so vast a body of deep warm water, several leagues wide, coming from between the tropics and issuing out of the gulf into the northern seas, should retain its warmth longer than the twenty or thirty days required to its passing the banks of Newfoundland. The quantity is too great, and it is too deep to be suddenly cooled by passing under a cooler air. The air immediately over it, however, may receive so much warmth from it as to be rarefied and rise, being rendered lighter than the air on each side of the stream; hence those airs must flow in to supply the place of the rising warm air, and, meeting with each other, form those tornados and waterspouts frequently met with, and seen near and over the stream; and as the vapor from a cup of tea in a warm room, and the breath of an animal in the same room, are hardly visible, but become sensible immediately when out in the cold air, so the vapor from the Gulf Stream, in warm latitudes, is scarcely visible, but when it comes into the cool air from Newfoundland, it is condensed into the fogs, for which those parts are so remarkable.

The power of wind to raise water above its common

level in the sea is known to us in America, by the high tides occasioned in all our seaports when a strong northeaster blows against the Gulf Stream.

The conclusion from these remarks is, that a vessel from Europe to North America may shorten her passage by avoiding to stem the stream, in which the thermometer will be very useful; and a vessel from America to Europe may do the same by the same means of keeping in it. It may have often happened accidentally, that voyages have been shortened by these circumstances. It is well to have the command of them.

But may there not be another cause, independent of winds and currents, why passages are generally shorter from America to Europe than from Europe to America? This question I formerly considered in the following short paper.

"On board the Pennsylvania Packet, Captain
"Osborne, at sea, 5 April, 1775.

"Suppose a ship to make a voyage eastward from a place in latitude 40° north, to a place in latitude 50° north, distance in longitude 75 degrees.

"In sailing from 40 to 50, she goes from a place where a degree of longitude is about eight miles greater than in the place she is going to. A degree is equal to four minutes of time; consequently the ship in the harbour she leaves, partaking of the diurnal motion of the earth, moves two miles in a minute faster, than when in the port she is going to; which is one hundred and twenty miles in an hour.

"This motion in a ship and cargo is of great force; and if she could be lifted up suddenly from the harbour in which she lay quiet, and set down instantly in the latitude of the port she was bound to, though in a calm, that force contained in her would make her run a great

way at a prodigious rate. This force must be lost gradually in her voyage, by gradual impulse against the water, and probably thence shorten the voyage. Query. In returning, does the contrary happen, and is her voyage thereby retarded and lengthened?"*

Would it not be a more secure method of planking ships, if, instead of thick single planks laid horizontally, we were to use planks of half the thickness, and lay them double and across each other, as in figure 23? To me it seems that the difference of expense would not be considerable, and that the ship would be both tighter and stronger.

The securing of the ship is not the only necessary thing; securing the health of the sailors, a brave and valuable order of men, is likewise of great importance. With this view the methods so successfully practised by Captain Cook, in his long voyages, cannot be too closely studied or carefully imitated. A full account of those methods is found in Sir John Pringle's speech, when the medal of the Royal Society was given to that illustrious navigator. I am glad to see in his last voyage, that he found the means effectual, which I had proposed for preserving flour, bread, &c., from moisture and damage. They were found dry and good after being at sea four years. The method is described in my printed works, page 452, fifth edition.† In the same, page 469, 470,‡ is proposed a means of allaying thirst in case of want of fresh water. This has since been practised in two instances with success. Happy

* Since this paper was read at the Society, an ingenious member, Mr. Patterson, has convinced the writer that the returning voyage would not, from this cause, be retarded.

† See the description referred to, Vol. V. p. 408.

‡ See above, p. 233.

if their hunger, when the other provisions are consumed, could be relieved as commodiously; and perhaps in time this may be found not impossible. An addition might be made to their present vegetable provision, by drying various roots in slices by the means of an oven. The sweet potato of America and Spain is excellent for this purpose. Other potatoes, with carrots, parsnips, and turnips, might be prepared and preserved in the same manner.

With regard to make-shifts in cases of necessity, seamen are generally very ingenious themselves. They will excuse, however, the mention of two or three. If they happen in any circumstance, such as after shipwreck, taking to their boat, or the like, to want a compass, a fine sewing needle laid on clear water in a cup will generally point to the north, most of them being a little magnetical, or may be made so by being strongly rubbed or hammered, lying in a north and south direction. If their needle is too heavy to float by itself, it may be supported by little pieces of cork or wood. A man who can swim, may be aided in a long traverse by his handkerchief formed into a kite, by two cross sticks extending to the four corners; which, being raised in the air when the wind is fair and fresh, will tow him along while lying on his back. Where force is wanted to move a heavy body, and there are but few hands and no machines, a long and strong rope may make a powerful instrument. Suppose a boat is to be drawn up on a beach, that she may be out of the surf; a stake drove into the beach where you would have the boat drawn, and another to fasten the end of the rope to, which comes from the boat, and then applying what force you have to pull upon the middle of the rope at right angles with it, the power will be augmented in proportion to the length of rope between the posts.

The rope being fastened to the stake, *A*, and drawn upon in the direction, *C D*, will slide over the stake *B*; and when the rope is bent to the angle, *A D B*, represented by the pricked line in figure 24, the boat will be at *B*.

Some sailors may think the writer has given himself unnecessary trouble in pretending to advise them; for they have a little repugnance to the advice of landmen, whom they esteem ignorant and incapable of giving any worth notice; though it is certain that most of their instruments were the invention of landmen. At least the first vessel ever made to go on the water was certainly such. I will therefore add only a few words more, and they shall be addressed to passengers.

When you intend a long voyage, you may do well to keep your intention as much as possible a secret, or at least the time of your departure; otherwise you will be continually interrupted in your preparations by the visits of friends and acquaintance, who will not only rob you of the time you want, but put things out of your mind, so that when you come to sea, you have the mortification to recollect points of business that ought to have been done, accounts you intended to settle, and conveniences you had proposed to bring with you, &c. &c., all which have been omitted through the effect of these officious friendly visits. Would it not be well if this custom could be changed; if the voyager, after having, without interruption, made all his preparations, should use some of the time he has left, in going himself to take leave of his friends at their own houses, and let them come to congratulate him on his happy return?

It is not always in your power to make a choice in your captain, though much of your comfort in the passage may depend on his personal character, as you must

for so long a time be confined to his company, and under his direction; if he be a sensible, sociable, good-natured, obliging man, you will be so much the happier. Such there are; but, if he happens to be otherwise, and is only skilful, careful, watchful, and active in the conduct of his ship, excuse the rest, for these are the essentials.

Whatever right you may have by agreement in the mass of stores laid in by him for the passengers, it is good to have some particular things in your own possession, so as to be always at your own command.

1. Good water, that of the ship being often bad. You can be sure of having it good only by bottling it from a clear spring or well, and in clean bottles. 2. Good tea. 3. Coffee, ground. 4. Chocolate. 5. Wine of the sort you particularly like, and cider. 6. Raisins. 7. Almonds. 8. Sugar. 9. Capillaire. 10. Lemons. 11. Jamaica spirits. 12. Eggs, greased. 13. Diet bread. 14. Portable soup. 15. Rusks. As to fowls, it is not worth while to have any called yours, unless you could have the feeding and managing of them according to your own judgment, under your own eye. As they are generally treated at present in ships, they are for the most part sick, and their flesh tough and hard as whit-leather. All seamen have an opinion, broached, I suppose, at first prudently, for saving of water when short, that fowls do not know when they have drunk enough, and will kill themselves if you give them too much, so they are served with a little only once in two days. This is poured into troughs that lie sloping, and therefore immediately runs down to the lower end. There the fowls ride upon one another's backs to get at it, and some are not happy enough to reach and once dip their bills in it. Thus tantalized, and tormented with thirst, they cannot digest their dry food, they fret, pine,

sicken, and die. Some are found dead, and thrown overboard every morning, and those killed for the table are not eatable. Their troughs should be in little divisions, like cups, to hold the water separately, figure 25. But this is never done. The sheep and hogs are therefore your best dependence for fresh meat at sea, the mutton being generally tolerable and the pork excellent.

It is possible your captain may have provided so well in the general stores, as to render some of the particulars above recommended of little or no use to you. But there are frequently in the ship poorer passengers, who are taken at a lower price, lodge in the steerage, and have no claim to any of the cabin provisions, or to any but those kinds that are allowed the sailors. These people are sometimes dejected, sometimes sick; there may be women and children among them. In a situation where there is no going to market to purchase such necessaries, a few of these your superfluities, distributed occasionally, may be of great service, restore health, save life, make the miserable happy, and thereby afford you infinite pleasure.

The worst thing in ordinary merchant ships is the cookery. They have no professed cook, and the worst hand as a seaman is appointed to that office, in which he is not only very ignorant but very dirty. The sailors have therefore a saying, that *God sends meat, and the Devil cooks.* Passengers more piously disposed, and willing to believe Heaven orders all things for the best, may suppose, that, knowing the sea air and constant exercise by the motion of the vessel would give us extraordinary appetites, bad cooks were kindly sent to prevent our eating too much; or that, foreseeing we should have bad cooks, good appetites were furnished to prevent our starving. If you cannot trust to these

circumstances, a spirit-lamp, with a blaze-pan, may enable you to cook some little things for yourself; such as a hash, a soup, &c. And it might be well also to have among your stores some potted meats, which, if well put up, will keep long good. A small tin oven, to place with the open side before the fire, may be another good utensil, in which your own servant may roast for you a bit of pork or mutton. You will sometimes be induced to eat of the ship's salt beef, as it is often good. You will find cider the best quencher of that thirst, which salt meat or fish occasions. The ... biscuit is too hard for some sets of teeth ... may be softened by toasting. But rusk is better, not being made of good fermented bread, sliced and baked a second time, the pieces imbibe the water easily, soften immediately, digest more kindly, and are therefore more wholesome than the unfermented biscuit. By the way, rusk is the true original biscuit, so prepared to keep for sea, biscuit in French signifying *twice baked*. If your dry peas boil hard, a two pound iron shot put with them into the pot will, by the motion of the ship, grind them as fine as mustard.

The accidents I have seen at sea with large dishes of soup upon a table, from the motion of the ship, have made me wish that our potters or pewterers would make soup dishes in divisions, like a set of small bowls united together, each containing about sufficient for one person, in some form as figure 26: for then, when the ship shoots ... a sudden heel the soup would not in a body ... fall into people's laps and scald them, as is sometimes the case, but would be retained in the ... figure 27.

After these trifles, permit the addition of a few general reflections. Navigation, when employed in supply-

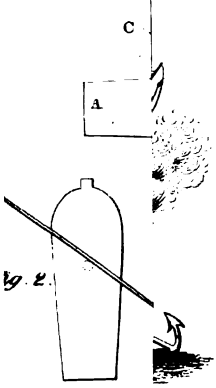


Fig. 7.

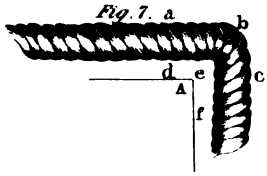


Fig. 8.

Fig. 9.

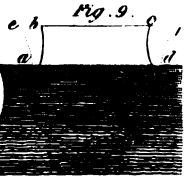
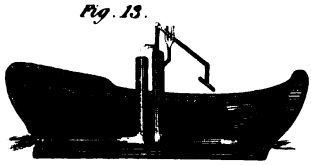


Fig. 2.

Fig. 13.



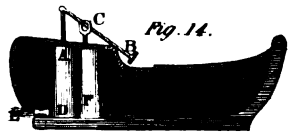
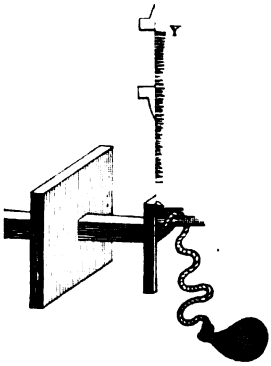


Fig. 14.



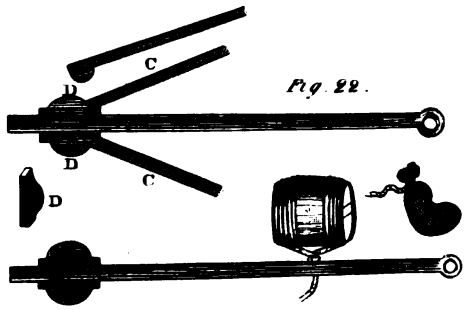


Fig. 22.

Fig. 26.

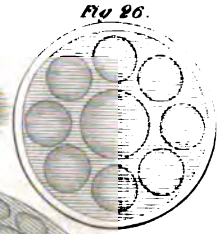


Fig. 19.

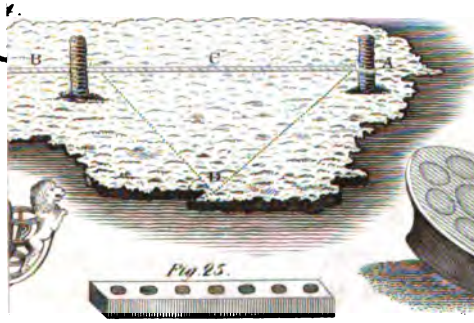


Fig. 25.

Fig. 27.

ing necessary provisions to a country in want, and thereby preventing famines, which were more frequent and destructive before the invention of that art, is undoubtedly a blessing to mankind. When employed merely in transporting superfluities, it is a question whether the advantage of the employment it affords is equal to the mischief of hazarding so many lives on the ocean. But when employed in pillaging merchants and transporting slaves, it is clearly the means of augmenting the mass of human misery. It is amazing to think of the ships and lives risked in fetching tea from China, coffee from Arabia, sugar and tobacco from America, all which our ancestors did well without. Sugar employs near one thousand ships, tobacco almost as many. For the utility of tobacco there is little to be said; and for that of sugar, how much more commendable would it be, if we could give up the few minutes' gratification afforded once or twice a day by the taste of sugar in our tea, rather than encourage the cruelties exercised in producing it. An eminent French moralist says, that when he considers the wars we excite in Africa to obtain slaves, the numbers necessarily slain in those wars, the many prisoners who perish at sea by sickness, bad provisions, foul air, &c. &c., in the transportation, and how many afterwards die from the hardships of slavery, he cannot look on a piece of sugar without conceiving it stained with spots of human blood! Had he added the consideration of the wars we make to take and retake the sugar islands from one another, and the fleets and armies that perish in those expeditions, he might have seen his sugar not merely spotted, but thoroughly dyed scarlet in grain. It is these wars, that make the maritime powers of Europe, the inhabitants of London and Paris, pay dearer for sugar than those of Vienna, a thousand miles from the sea; be-

cause their sugar costs not only the price they pay for it by the pound, but all they pay in taxes to maintain the fleets and armies that fight for it.*

With great esteem, I am, Sir,
Your most obedient humble servant,
B. FRANKLIN.

APPENDIX TO THE ABOVE LETTER.

Remarks upon the Navigation from Newfoundland to New York, in Order to avoid the Gulf Stream on one Hand, and, on the other, the Shoals that lie to the southward of Nantucket and of St. George's Banks.

READ AT A MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY, DECEMBER 2D, 1785.

After you have passed the banks of Newfoundland in about the forty-fourth degree of latitude, you will meet with nothing, till you draw near the Isle of Sables, which we commonly pass in latitude 43°. Southward of this Isle, the current is found to extend itself as far north as 41° 20′ or 30′, then it turns towards the E. S. E. or S. E. $\frac{1}{4}$ E.

Having passed the Isle of Sables, shape your course for the St. George's Banks, so as to pass them in about latitude 40°, because the current southward of those banks reaches as far north as 39°. The shoals of those banks lie in 41° 35′.

After having passed St. George's Banks, you must, to clear Nantucket, form your course so as to pass between the latitudes 38° 30′ and 40° 45′.

The most southern part of the shoals of Nantucket

* Several of the closing paragraphs of this letter are the same in substance, as the piece entitled, "*Precautions to be used by those, who are about to undertake a Sea Voyage*," Vol. II. p. 106. — EDITOR.

lie in about 40° 45′. The northern part of the current directly to the south of Nantucket is felt in about latitude 38° 30′.

By observing these directions, and keeping between the stream and the shoals, the passage from the Banks of Newfoundland to New York, Delaware, or Virginia, may be considerably shortened; for so you will have the advantage of the eddy current, which moves contrary to the Gulf Stream. Whereas, if to avoid the shoals you keep too far to the southward, and get into that stream, you will be retarded by it at the rate of sixty or seventy miles a day.

The Nantucket whalemen being extremely well acquainted with the Gulf Stream, its course, strength, and extent, by their constant practice of whaling on the edges of it, from their island quite down to the Bahamas, this draft of that stream [Plate XIII.] was obtained from one of them, Captain Folger, and caused to be engraved on the old chart in London, for the benefit of navigators, by

B. FRANKLIN.

NOTE. The Nantucket captains, who are acquainted with this stream, make their voyages from England to Boston in as short a time generally as others take in going from Boston to England, viz. from twenty to thirty days.

A stranger may know when he is in the Gulf Stream, by the warmth of the water, which is much greater than that of the water on each side of it. If then he is bound to the westward, he should cross the stream to get out of it as soon as possible.

B. FRANKLIN.

Observations of the Warmth of the Sea Water, &c., by Fahrenheit's Thermometer, in crossing the Gulf Stream; with other Remarks made on board the Pennsylvania Packet, Captain Osborne, bound from London to Philadelphia, in April and May, 1775.

Date.	Hour.	Temp. of Air.	Temp. of Water.	Wind.	Course.	Dis-tance.	Latitude N.	Longitude W.	Remarks.
April 10			62						
11			61						
12			64						
13			65						
14			65						
26			70	S. S. E.	W. b. S.		37° 39′	60° 39′	Much gulf weed; saw a whale.
27		60	70	S. W.	W. N. W.		37 13	62 29	Color of water changed.
28	8 A. M.	60	64				37 48	64 35	No gulf weed.
	6 P. M.	67	60			34			Sounded, no bottom.
29	8 A. M.	63	71	N. E.	W.	44	37 26	66 0	Much light in the water last night.
	5 P. M.	65	72	N. E.					Water again of the usual deep sea-color, little or no light in it at night.
	11 "	66	66	N. W. b. N.	W. b. S.	57			
30	8 A. M.	64	70	N. E.	W. b. N.	60	37 20	68 53	Frequent gulf weed, water continues of sea-color, little light.
	12	62	70		E. b. S.	24			
	6 P. M.	64	72	E. S. E.	W. b. N.	43			Much light.
	10 "	65	66	S.		25			
May 1	7 A. M.	68	63	S. S. W.	W. N. W.	60	38 13	72 23	Much light all last night.
	12	65	56		W. b. N.	44			Color of water changed.
	4 P. M.	64	56	S. W.	W. N. W.	21			Much light.
	10 "	64	57			31			
2	8 A. M.	62	53	W. S. W.	N. W.	18	38 43	74 3	Much light. Thunder-gust.
	12	60	53	N. W.	W. S. W.	18			
	6 P. M.	64	55	N. b. W.	W. b. N.	15			
	10 "	65	55			10			
3	7 A. M.	62	54			30	38 30	75 0	

Observations of the Warmth of the Sea Water, &c., by Fahrenheit's Thermometer; with other Remarks made on board the Reprisal, Captain Wickes, bound from Philadelphia to France, in October and November, 1776.

Date.	Hour A. M.	Hour P. M.	Temp. of Air.	Temp. of Water.	Wind.	Course.	Distance.	Latitude N.	Longitude W.	Remarks.
Oct. 31	10	4	76	70	S. S. E.	E. b. S.	135	39° 12′	70° 30′	Left the capes Thursday night, October 29, 1776.
Nov. 1	10	4		71	W. S. W.	E. ½ N.	109	No obs.	68 12	
2	8		71	78						
	12		71	81	N.					
				75						
3	8	4	67	78			141	Do.	65 23	
	12			76	N. W.	E. S. E. ½ E.				Some sparks in the water these two last nights.
				76		E. b. S.	160	37 0	62 7	
4	9	4	70	76						Do.
			68	76		N. b. E.	194	36 26	58 8	
		1		76						
		4	68	78						
		8		76						
5	8		68	75		N. E.	163	35 21	55 3	Do.
	12	4	70	75						
		8		75						
6	8			76	E. b. N.	S. 50 E.	75	35 33	53 52	
7	12			77						
8	8			78	S. E. b. E.	N. 30 W.	108	36 6	52 46	
	12	4		77						
8	9		75	77	S. b. E.	N. 49 E.	175	38 2	50 1	
	12	4		77						
9	9		75	77	S. W.	N 33 E.	175	39 39	46 55	
	12		75	70						

Observations made on board the Reprisal, continued.

Date.	Hour A. M.	Hour P. M.	Temp. of Air.	Temp. of Water.	Wind.	Course.	Dis-tance.	Latitude N.	Longitude W.	Remarks.
Nov. 9		4	70	71	E.	N. 17 E.	64	40° 39′	46° 27′	
10	8			68						
	12			64						
11	8			63	S. E.	N. 8 E.	41	41 19	46 19	
	12			61						
12	8		56	59	N. N. W.	N. 80 E.	120	41 39	43 43	
		4		69	E.	S. 82 E.	69	41 29	43 10	
13	all	day		68						
14	8		70	70	E. S. E.	N. 74 E.	111	43 0	39 57	
		noon		72						
		4		71						
15	8		61	69	W. S. W.	N. 70 E.	186	43 3	35 51	
		noon		68						
		4		67						
16		noon	65	67	S. W.	N. 67 W.	48	43 22	34 50	
		4		63						
17	8			63	E. S. E.	N. 19 E.	56	44 15	34 25	
18	all			65	S. b. W.	N. 75 E.	210	45 6	29 43	Some gulf weed.
19		day		64	S. W.	N. 80 E.	228	45 46	24 2	
20	8	noon	65	62	N.	S. 80 E.	155	45 19	20 30	
		4		60						
21	9			62	S.	N. 88 E.	94	45 22	18 17	
22	10			62	S. S. W.	S. 89 E.	133	45 19	15 19	
23			60	61	W. b. W.	S. 86 E.	194	45 6	10 35	
24		noon		60	N. N. E.	N. 78 E.	191	45 46	6 10	
25		do.		60	N. E.	S. 76 E.	125	45 4	3 23	
26		do.	56	60						
27		do.		58	E.	N. 73 E.	31	45 13	2 20	Soundings off Belisle.
28		do.	54	56						

A Journal of a Voyage from the Channel between France and England towards America in 1785.

Dates.	Latitude N.	Longitude W.	Therm. A. M.		Therm. P. M.		Winds.	Course.	Distance.	Variation of the Needle.	Therm. Noon.	
			Air.	Water.	Air.	Water.					Air.	Water.
July 29	49° 15′	4° 15′	63	57	63	58	{ These are average of	taken on an 24 hours.	Miles.	West. 22° 0′		
30	48 28	8 58	63	58	62	62	E.	S. W. ½ W.	60			
31	47 0	12 13	64	62	64	64	E. S. E.	W. b. S. ½ S.	174			
Aug. 1	45 0	15 43	60	64	60	63	N. E.	S. W. b. W.	160			
2	43 5	17 25	68	67	omit	ted	N. W. b. W.	S. W. ½ W.	190			
3	41 3	19 44	67	66	do.	66	N. E.	S. W. b. S.	131	20 0		
4	38 45	21 34	70	65	65	68	N. E.	S. W. ½ S.	166	16 30		
5	36 42	23 10	70	68	71	69	N. E.	S. W. ½ W.	165	11 30		
6	35 40	25 40	71	70	68	70	N. E.	S. S. W. ½ W.	149	11 15		
7	35 0	27 0	72	72	73	72	N. E.	W. S. W. ½ S.	137			
8	33 51	28 43	73	74	73	74	N. E.	W. S. W. ½ W.	76			
9	33 30	31 30	71	73	77	75	N.	S. W. ½ W.	112			
10	33 17	33 38	74	73	76	77	N.	W. ½ S.	143			
11	33 23	34 31	76	74	76	76	N. E.	W. ½ S.	103			
12	33 45	35 0	76	75	78	77	S. E.	W. ½ S.	50			
13	34 14	35 30	78	76	79	79	W. N. W.	S. W. ½ N.	35			
14	35 37	36 4	79	78	81	80	W.	N. W. ½ N.	38			
15	36 7	37 16	80	78	80	78	W. S. W.	N. N. W.	75			
16	36 38	38 0	80	77	78	ted	N. W. b. W.	N. W. ½ N.	65			
17							W. S. W.	N. W. ½ W.	49			
18												
19												

Journal of a Voyage, &c., continued.

Dates.	Latitude N.	Longitude W.	Therm. A. M.		Therm. P. M.		Winds.	Course.	Distance.	Variation of the Needle.	Therm. Noon.	
			Air.	Water.	Air.	Water.					Air.	Water.
	° ′	° ′										
Aug. 20	37 39	39 6	78	76	omitted	76	W.	N. ½ W.	62		77	75
21	36 15	38 26	73	74	78	76	W. N. W.	S. b. W.	32		77	75
22	35 40	38 44	77	76	80	77	W. b. S.	S. S. W.	38		80	77
23	35 35	40 52	79	77	78	75	N.	W. ½ S.	100		omitted.	
24	35 12	41 31	75	73	75	74	W. N. W.	S. W. b. W.	41		75	74
25	35 40	42 33	79	76	79	76	W. b. N.	W. N. W. ½ N.	60		80	76
26	35 30	43 44	79	76	80	76	S. W. b. W.	S. W. ½ S.	14		80	76
27	35 14	43 23	79	77	81	79	W.	W. ½ S.	38		81	78
28	34 23	44 0	78	76	78	78	N. N. E.	S. W. b. S.	60	9 0	78	78
29	34 12	45 52	77	78	78	78	N. E.	W. ½ S.	04		79	78
30	34 6	48 31	79	78	78	78	E.	W. ½ S.	134		78	78
31	34 20	51 4	80	79	81	79	E.	W. ½ S.	129		80	80
Sept. 1	34 20	52 47	81	78	omitted		S. S. W	W. ½ N.	96		83	80
2	34 55	55 12	81	80	83	80	S. W.	W. b. N. ½ W.	125		83	80
3	35 30	57 24	83	80	83	80	S. W. b. S.	W. b. N. ½ N.	114		84	81
4	35 50	59 1	82	80	82	80	S. W. ½ W.	W. b. N. ½ N.	82	6 0	83	81
5	35 55	61 0	81	80	82	81	S. S. W.	W. ½ N.	96		82	81
6	36 20	62 30	80	81	79	80	N. W. b. N.	W. b. N.	75		78	80
7	34 50	63 10	87	80	78	81	N. W. b. N.	S. S. W.	86		78	81
8	34 45	64 40	75	79	75	79	N.	W. ½ S.	74		75	79
9	35 43	66 42	77	79	77	73	N. E.	W. N. W.	106		78	80
10	37 20	68 40	77	73	77	70	E. N. E.	N. W.	126		78	72

Observations.

July 31. At one P. M. the Start bore W. N. W. distant six leagues.
August 1. The water appears luminous in the ship's wake.
— 2. The temperature of the water is taken at eight in the morning and at eight in the evening.
— 6. The water appears less luminous.
— 7. Formegas S. W. distant 32½ degrees. St. Mary's S W. ½ S. 33 leagues.
— 8. From this date the temperature of the water is taken at eight in the morning and at six in the evening.
— 10. Moonlight, which prevents the luminous appearance of the water.
— 11. A strong southerly current.
— 12. Ditto. From this date the temperature of the air and water was taken at noon, as well as morning and evening.
— 16. Northerly current.
— 19. First saw gulf weed.
— 21 Southerly current.
— 22. Again saw gulf weed.
— 24. The water appeared luminous in a small degree before the moon rose.
— 29. No moon, yet very little light in the water.
— 33. Much gulf weed to-day.
— 31. Ditto.
September 1. Ditto.
— 2. A little more light in the water.
— 4. No gulf weed to-day. More light in the water
— 5. Some gulf weed again.
— 6. Little light in the water. A very hard thunder-gust in the night.
— 7. Little gulf weed.
— 8. More light in the water. Little gulf weed.
— 9. Little gulf weed. Little light in the water last evening.
— 10. Saw some beds of rock-weed; and we were surprised to observe the water six degrees colder by the thermometer than the preceding noon.

This day (10th) the thermometer still kept descending, and at five in the morning of the 11th, it was in water as low as 70, when we struck soundings. The same evening the pilot came on board, and we found our ship about five degrees of longitude a-head of the reckoning, which our captain accounted for by supposing our course to have been near the edge of the Gulf Stream, and thus an eddy-current always in our favor. By the distance we ran from September 9th, in the evening, till we struck soundings, we must have then been at the western edge of the Gulf Stream, and the change in the temperature of the

water was probably owing to our suddenly passing from that current into the waters of our own climate.

On the 14th of August the following experiment was made. The weather being perfectly calm, an empty bottle, corked very tight, was sent down 20 fathoms, and it was drawn up still empty. It was then sent down again 35 fathoms, when the weight of the water having forced in the cork, it was drawn up full; the water it contained was immediately tried by the thermometer, and found to be 70, which was six degrees colder than at the surface; the lead and bottle were visible, but not very distinctly so, at the depth of 12 fathoms; but, when only 7 fathoms deep, they were perfectly seen from the ship. This experiment was thus repeated September 11th, when we were in soundings of 18 fathoms. A keg was previously prepared with a valve at each end, one opening inward, the other outward; this was sent to the bottom in expectation that by the valves being both open when going down, and both shut when coming up, it would keep within it the water received at bottom. The upper valve performed its office well, but the under one did not shut quite close, so that much of the water was lost in hauling it up the ship's side. As the water in the keg's passage upwards could not enter at the top, it was concluded that what water remained in it was of that near the ground; and, on trying this by the thermometer, it was found to be at 58, which was 12 degrees colder than at the surface.

[This last Journal was obligingly kept for me by Mr. J. Williams, my fellow-passenger in the London Packet, who made all the experiments with great exactness.]*

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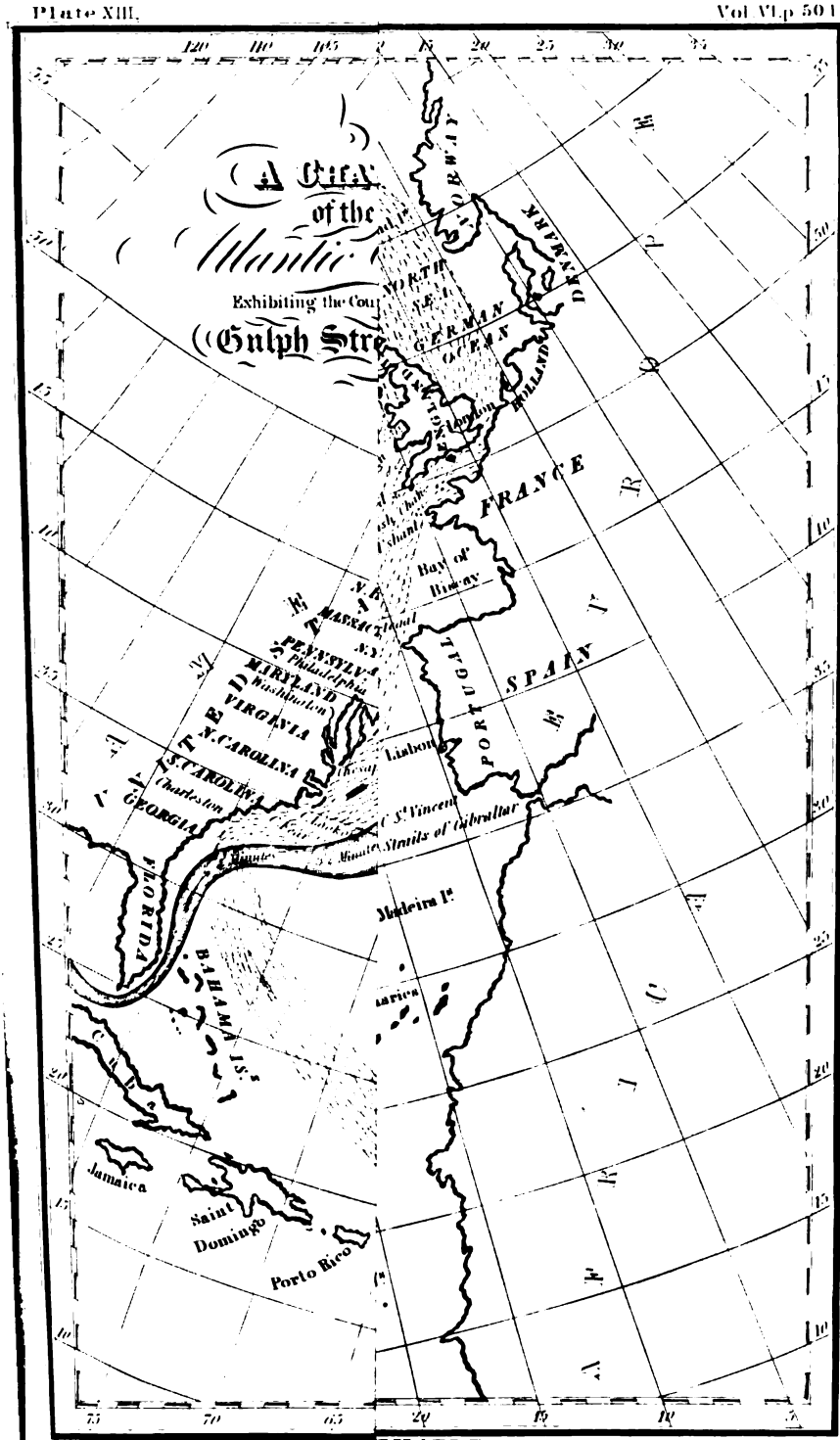
* The chart given in this edition has been constructed with a view to give a more comprehensive idea of the course of the Gulf Stream. Volney very plausibly suggests, that the earth, deposited by the Gulf Stream south-east of Newfoundland, has formed the great banks; and that the accumulation there has given the stream a new or more eastwardly direction. This chart also serves to illustrate the long-received ideas of the progress of the shoals of fish. May not the glutinous matter seen on the water, and which all persons who have been across the line must have noticed, be another cause of the phenomena of fish shoals. May they not come in search of the food, which the matter seen on the water in such abundance affords? The writer of this note has observed, that, on entering the trade winds, the seamen have judged of the change of wind approaching by the direction of the bonetta and other fish, which pass in shoals in the South Atlantic and south-eastern Seas, in a direction opposite to the wind; and when not opposite to the prevailing wind, they conclude a change to be at hand from the direction towards which the fish go. The appearance of luminous floating matter at night is often followed by shoals of fish; the spawn or gluten, which the writer has had taken up in a bucket, has been often found as large as two inches diameter, and frequently induced an opinion that it was a species of maritime *cocoon* or egg of an animal. Fragments of irregular shaped gluten have been also often seen. An inquiry into the


A Chart
of the
Atlantic
Exhibiting the Cou
Gulph Stre
NORTH SEA
GERMAN OCEAN
DENMARK
HOLLAND
FRANCE
Bay of Biscay
PORTUGAL
SPAIN
MASSACHUSETS
PENNSYLVA
Philadelphia
MARYLAND
Washington
VIRGINIA
N. CAROLINA
S. CAROLINA
Charleston
GEORGIA
FLORIDA
Lisbon
C. St Vincent
Straits of Gibraltar
Madeira Is
BAHAMA ISS
Jamaica
Saint Domingo
Porto Rico

TO JOHN INGENHOUSZ, AT VIENNA.*

On the Causes and Cure of Smoky Chimneys.

At Sea, 28 August, 1785.

DEAR FRIEND,

In one of your letters, a little before I left France, you desire me to give you in writing my thoughts upon the construction and use of chimneys, a subject you had sometimes heard me touch upon in conversation. I embrace willingly this leisure afforded by my present situation to comply with your request, as it will not only show my regard to the desires of a friend, but may at the same time be of some utility to others; the doctrine of chimneys appearing not to be as yet generally well understood, and mistakes respecting them being attended with constant inconvenience, if not remedied, and with fruitless expense, if the true remedies are mistaken.

Those, who would be acquainted with this subject, should begin by considering on what principle smoke ascends in any chimney. At first, many are apt to think, that smoke is in its nature and of itself specifically lighter than air, and rises in it for the same reason that cork rises in water. These see no cause why smoke

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periodical appearance of these luminous substances on voyages to the southward, and remarks on the usual direction of the shoals of bonetta and other fish, might perhaps lead to very interesting discoveries. It might be assumed as a question worthy of examination, whether the direction of shoals of fish is not towards those points from which periodical winds or currents move the waters; and if the shoals of fish, which move from the north poles, and by the British Isles across the Atlantic, are not led by their instinct in search of these periodical supplies of food; and if the deposits made by the Gulf Stream on the banks of Newfoundland are not the true cause of the great abundance of fish found there.— DUANE.

* This letter, which has been published in a separate pamphlet, both in England and America, first appeared in the Transactions of the American Philosophical Society, in which it was read October 21st, 1785. —EDITOR.

should not rise in the chimney, though the room be ever so close. Others think there is a power in chimneys to *draw* up the smoke, and that there are different forms of chimneys, which afford more or less of this power. These amuse themselves with searching for the best form. The equal dimensions of a funnel in its whole length are not thought artificial enough, and it is made, for fancied reasons, sometimes tapering and narrowing from below upwards, and sometimes the contrary, &c. &c. A simple experiment or two may serve to give more correct ideas. Having lit a pipe of tobacco, plunge the stem to the bottom of a decanter half filled with cold water; then putting a rag over the bowl, blow through it and make the smoke descend in the stem of the pipe, from the end of which it will rise in bubbles through the water; and, being thus cooled, will not afterwards rise to go out through the neck of the decanter, but remain spreading itself and resting on the surface of the water. This shows that smoke is really heavier than air, and that it is carried upwards only when attached to, or acted upon, by air that is heated, and thereby rarefied and rendered specifically lighter than the air in its neighbourhood.

Smoke being rarely seen but in company with heated air, and its upward motion being visible, though that of the rarefied air that drives it is not so, has naturally given rise to the error.

I need not explain to you, my learned friend, what is meant by rarefied air; but, if you make the public use you propose of this letter, it may fall into the hands of some who are unacquainted with the term and with the thing. These then may be told, that air is a fluid which has weight as well as others, though about eight hundred times lighter than water. That heat makes the particles of air recede from each other and take up

more space, so that the same weight of air heated will have more bulk, than equal weights of cold air which may surround it, and in that case must rise, being forced upwards by such colder and heavier air, which presses to get under it and take its place. That air is so rarefied or expanded by heat may be proved to their comprehension by a lank blown bladder, which, laid before a fire, will soon swell, grow tight, and burst.

Another experiment may be, to take a glass tube about an inch in diameter, and twelve inches long, open at both ends and fixed upright on legs, so that it need not be handled, for the hands might warm it. At the end of a quill fasten five or six inches of the finest light filament of silk, so that it may be held either above the upper end of the tube or under the lower end, your warm hand being at a distance by the length of the quill. (Plate XIV. Fig. 1.) If there were any motion of air through the tube, it would manifest itself by its effect on the silk; but, if the tube and the air in it are of the same temperature with the surrounding air, there will be no such motion, whatever may be the form of the tube, whether crooked or strait, narrow below and widening upwards, or the contrary; the air in it will be quiescent. Warm the tube, and you will find, as long as it continues warm, a constant current of air entering below and passing up through it, till discharged at the top; because the warmth of the tube, being communicated to the air it contains, rarefies that air and makes it lighter than the air without, which therefore presses in below, forces it upwards, and follows and takes its place, and is rarefied in its turn. And, without warming the tube, if you hold under it a knob of hot iron, the air thereby heated will rise and fill the tube, going out at its top; and this motion in the tube will continue as long as the knob remains hot, because the air enter-

ing the tube below is heated and rarefied by passing near and over that knob.

That this motion is produced merely by the difference of specific gravity between the fluid within and that without the tube, and not by any fancied form of the tube itself, may appear by plunging it into water contained in a glass jar a foot deep, through which such motion might be seen. The water within and without the tube being of the same specific gravity, balance each other, and both remain at rest. But take out the tube, stop its bottom with a finger and fill it with olive oil, which is lighter than water, then stopping the top, place it as before, its lower end under water, its top a very little above. As long as you keep the bottom stopped, the fluids remain at rest, but the moment it is unstopped, the heavier enters below, forces up the lighter, and takes its place. And the motion then ceases, merely because the new fluid cannot be successively made lighter, as air may be by a warm tube.

In fact, no form of the funnel of a chimney has any share in its operation or effect respecting smoke, except its height. The longer the funnel, if erect, the greater its force when filled with heated and rarefied air, to *draw* in below and drive up the smoke, if one may, in compliance with custom, use the expression *draw*, when in fact it is the superior weight of the surrounding atmosphere that *presses* to enter the funnel below, and so *drives up* before it the smoke and warm air it meets with in its passage.

I have been the more particular in explaining these first principles, because, for want of clear ideas respecting them, much fruitless expense has been occasioned; not only single chimneys, but, in some instances within my knowledge, whole stacks having been pulled down and rebuilt with funnels of different forms, imagined

more powerful in *drawing* smoke; but, having still the same height and the same opening below, have performed no better than their predecessors.

What is it then which makes a *smoky chimney;* that is, a chimney which, instead of conveying up all the smoke, discharges a part of it into the room, offending the eyes and damaging the furniture?

The causes of this effect, which have fallen under my observation, amount to *nine*, differing from each other, and therefore requiring different remedies.

1. *Smoky chimneys in a new house are such, frequently, from mere want of air.* The workmanship of the rooms being all good, and just out of the workman's hand, the joints of the boards of the flooring, and of the pannels of wainscoting are all true and tight, the more so as the walls, perhaps not yet thoroughly dry, preserve a dampness in the air of the room, which keeps the wood work swelled and close. The doors and the sashes too, being worked with truth, shut with exactness, so that the room is as tight as a snuffbox, no passage being left open for air to enter, except the keyhole, and even that is sometimes covered by a little dropping shutter. Now, if smoke cannot rise but as connected with rarefied air, and a column of such air, suppose it filling the funnel, cannot rise, unless other air be admitted to supply its place; and if, therefore, no current of air enter the opening of the chimney, there is nothing to prevent the smoke coming out into the room. If the motion upwards of the air in a chimney, that is freely supplied, be observed by the rising of the smoke or a feather in it, and it be considered, that, in the time such feather takes in rising from the fire to the top of the chimney, a column of air equal to the content of the funnel must be discharged, and an equal quantity supplied from the room below, it will appear

absolutely impossible that this operation should go on if the tight room is kept shut; for, were there any force capable of drawing constantly so much air out of it, it must soon be exhausted like the receiver of an air-pump, and no animal could live in it. Those therefore who stop every crevice in a room to prevent the admission of fresh air, and yet would have their chimney carry up the smoke, require inconsistencies, and expect impossibilities. Yet, under this situation, I have seen the owner of a new house, in despair, and ready to sell it for much less than it cost, conceiving it uninhabitable, because not a chimney in any one of its rooms would carry off the smoke, unless a door or window were left open. Much expense has also been made, to alter and amend new chimneys which had really no fault; in one house particularly that I knew, of a nobleman in Westminster, that expense amounted to no less than three hundred pounds, *after* his house had been, as he thought, finished and all charges paid. And after all, several of the alterations were ineffectual, for want of understanding the true principles.

Remedies. When you find on trial, that opening the door or a window enables the chimney to carry up all the smoke, you may be sure that want of air *from without* was the cause of its smoking. I say *from without*, to guard you against a common mistake of those, who may tell you the room is large, contains abundance of air, sufficient to supply any chimney, and therefore it cannot be that the chimney wants air. These reasoners are ignorant, that the largeness of a room, if tight, is in this case of small importance, since it cannot part with a chimney full of its air without occasioning so much vacuum; which it requires a great force to effect, and could not be borne, if effected.

It appearing plainly, then, that some of the outward

air must be admitted, the question will be, how much is *absolutely necessary;* for you would avoid admitting more, as being contrary to one of your intentions in having a fire, viz. that of warming your room. To discover this quantity, shut the door gradually while a middling fire is burning, till you find, that, before it is quite shut, the smoke begins to come out into the room, then open it a little till you perceive the smoke comes out no longer. There hold the door, and observe the width of the open crevice between the edge of the door and the rabbet it should shut into. Suppose the distance to be half an inch, and the door eight feet high, you find thence that your room requires an entrance for air equal in area to ninety-six half inches, or forty-eight square inches, or a passage of six inches by eight. This however is a large supposition, there being few chimneys, that, having a moderate opening and a tolerable height of funnel, will not be satisfied with such a crevice of a quarter of an inch; and I have found a square of six by six, or thirty-six square inches, to be a pretty good medium, that will serve for most chimneys. High funnels, with small and low openings, may indeed be supplied through a less space, because, for reasons that will appear hereafter, the *force of levity,* if one may so speak, being greater in such funnels, the cool air enters the room with greater velocity, and consequently more enters in the same time. This however has its limits, for experience shows, that no increased velocity, so occasioned, has made the admission of air through the keyhole equal in quantity to that through an open door; though through the door the current moves slowly, and through the keyhole with great rapidity.

It remains then to be considered how and where this necessary quantity of air from without is to be admitted, so as to be least inconvenient. For, if at the

door, left so much open, the air thence proceeds directly to the chimney, and in its way comes cold to your back and heels as you sit before your fire. If you keep the door shut, and raise a little the sash of your window, you feel the same inconvenience. Various have been the contrivances to avoid this, such as bringing in fresh air through pipes in the jambs of the chimney, which, pointing upwards, should blow the smoke up the funnel; opening passages into the funnel above, to let in air for the same purpose. But these produce an effect contrary to that intended; for, as it is the constant current of air passing from the room *through the opening of the chimney* into the funnel, which prevents the smoke coming out into the room, if you supply the funnel by other means or in other ways with the air it wants, and especially if that air be cold, you diminish the force of that current, and the smoke in its effort to enter the room finds less resistance.

The wanted air must then *indispensably* be admitted into the room, to supply what goes off through the opening of the chimney. M. Gauger, a very ingenious and intelligent French writer on the subject, proposes with judgment to admit it *above* the opening of the chimney; and to prevent inconvenience from its coldness, he directs its being made to pass in its entrance through winding cavities made behind the iron back and sides of the fireplace, and under the iron hearth-plate; in which cavities it will be warmed, and even heated, so as to contribute much, instead of cooling, to the warming of the room. This invention is excellent in itself, and may be used with advantage in building new houses; because the chimneys may then be so disposed, as to admit conveniently the cold air to enter such passages; but in houses built without such views, the chimneys are often so situated, as not to afford that

convenience, without great and expensive alterations. Easy and cheap methods, though not quite so perfect in themselves, are of more general utility; and such are the following.

In all rooms where there is a fire, the body of air warmed and rarefied before the chimney is continually changing place, and making room for other air that is to be warmed in its turn. Part of it enters and goes up the chimney, and the rest rises and takes place near the ceiling. If the room be lofty, that warm air remains above our heads as long as it continues warm, and we are little benefited by it, because it does not descend till it is cooler. Few can imagine the difference of climate between the upper and lower parts of such a room, who have not tried it by the thermometer, or by going up a ladder till their heads are near the ceiling. It is then among this warm air that the wanted quantity of outward air is best admitted, with which being mixed, its coldness is abated, and its inconvenience diminished so as to become scarce observable. This may be easily done, by drawing down about an inch the upper sash of a window; or, if not movable, by cutting such a crevice through its frame; in both which cases, it will be well to place a thin shelf of the length, to conceal the opening, and sloping upwards to direct the entering air horizontally along and under the ceiling. In some houses the air may be admitted by such a crevice made in the wainscot, cornice, or plastering, near the ceiling and over the opening of the chimney. This, if practicable, is to be chosen, because the entering cold air will there meet with the warmest rising air from before the fire, and be soonest tempered by the mixture. The same kind of shelf should also be placed here. Another way, and not a very difficult one, is to take out an upper pane of glass in one of your sashes, set in a tin

frame, (Plate, Fig. 2,) giving it two springing angular sides, and then replacing it, with hinges below on which it may be turned to open more or less above. It will then have the appearance of an internal skylight. By drawing this pane in, more or less, you may admit what air you find necessary. Its position will naturally throw that air up and along the ceiling. This is what is called in France a *Was ist das?* As this is a German question, the invention is probably of that nation, and takes its name from the frequent asking of that question when it first appeared. In England, some have of late years cut a round hole about five inches diameter in a pane of the sash, and placed against it a circular plate of tin hung on an axis, and cut into vanes, which, being separately bent a little obliquely, are acted upon by the entering air, so as to force the plate continually round like the vanes of a windmill. This admits the outward air, and by the continual whirling of the vanes, does in some degree disperse it. The noise, only, is a little inconvenient.

2. A second cause of the smoking of chimneys is, *their openings in the room being too large;* that is, too wide, too high, or both. Architects in general have no other ideas of proportion in the opening of a chimney, than what relate to symmetry and beauty, respecting the dimensions of the room;* while its true proportion, respecting its function and utility, depends on quite other principles; and they might as properly proportion the step in a staircase to the height of the story, instead of the natural elevation of men's legs in mounting. The proportion then to be regarded, is what relates to the height of the funnel. For as the funnels in the different stories of a house are necessarily of different

* See Notes at the end of the letter, No. I.

heights or lengths, that from the lowest floor being the highest or longest, and those of the other floors shorter and shorter, till we come to those in the garrets, which are of course the shortest; and the force of draft being, as already said, in proportion to the height of funnel filled with rarefied air; and a current of air from the room into the chimney, sufficient to fill the opening, being necessary to oppose and prevent the smoke coming out into the room; it follows, that the openings of the longest funnels may be larger, and that those of the shorter funnels should be smaller. For, if there be a large opening to a chimney that does not draw strongly, the funnel may happen to be furnished with the air it demands by a partial current entering on one side of the opening, and, leaving the other side free of any opposing current, may permit the smoke to issue there into the room. Much, too, of the force of draft in a funnel depends on the degree of rarefaction in the air it contains, and that depends on the nearness to the fire of its passage in entering the funnel. If it can enter far from the fire on each side, or far above the fire, in a wide or high opening, it receives little heat in passing by the fire, and the contents of the funnel are by that means less different in levity from the surrounding atmosphere, and its force in drawing consequently weaker. Hence, if too large an opening be given to chimneys in upper rooms, those rooms will be smoky; on the other hand, if too small openings be given to chimneys in the lower rooms, the entering air, operating too directly and violently on the fire, and afterwards strengthening the draft as it ascends the funnel, will consume the fuel too rapidly.

Remedy. As different circumstances frequently mix themselves in these matters, it is difficult to give precise dimensions for the openings of all chimneys. Our

fathers made them generally much too large; we have lessened them; but they are often still of greater dimension than they should be, the human eye not being easily reconciled to sudden and great changes. If you suspect that your chimney smokes from the too great dimension of its opening, contract it by placing movable boards so as to lower and narrow it gradually, till you find the smoke no longer issues into the room. The proportion so found will be that which is proper for that chimney, and you may employ the bricklayer or mason to reduce it accordingly. However, as, in building new houses, something must be sometimes hazarded, I would make the openings in my lower rooms about thirty inches square and eighteen deep, and those in the upper, only eighteen inches square and not quite so deep; the intermediate ones diminishing in proportion as the height of funnel diminished. In the larger openings, billets of two feet long, or half the common length of cord wood, may be burnt conveniently; and for the smaller, such wood may be sawed into thirds. Where coals are the fuel, the grates will be proportioned to the openings. The same depth is nearly necessary to all, the funnels being all made of a size proper to admit a chimney sweeper. If, in large and elegant rooms, custom or fancy should require the appearance of a larger chimney, it may be formed of expensive marginal decorations, in marble, &c. In time, perhaps, that which is fittest in the nature of things may come to be thought handsomest. But at present, when men and women in different countries show themselves dissatisfied with the forms God has given to their heads, waists, and feet, and pretend to shape them more perfectly, it is hardly to be expected that they will be content always with the best form of a chimney. And there are some, I know, so bigoted to the fancy of a

large noble opening, that rather than change it, they would submit to have damaged furniture, sore eyes, and skins almost smoked to bacon.

3. Another cause of smoky chimneys is *too short a funnel.* This happens necessarily in some cases, as where a chimney is required in a low building; for, if the funnel be raised high above the roof, in order to strengthen its draft, it is then in danger of being blown down, and crushing the roof in its fall.

Remedies. Contract the opening of the chimney, so as to oblige all the entering air to pass through or very near the fire; whereby it will be more heated and rarefied, the funnel itself be more warmed, and its contents have more of what may be called the force of levity, so as to rise strongly, and maintain a good draft at the opening.

Or you may in some cases, to advantage, build additional stories over the low building, which will support a high funnel.

If the low building be used as a kitchen, and a contraction of the opening therefore inconvenient, a large one being necessary, at least when there are great dinners, for the free management of so many cooking utensils; in such case, I would advise the building of two more funnels joining to the first, and having three moderate openings, one to each funnel, instead of one large one. When there is occasion to use but one, the other two may be kept shut by sliding plates, hereafter to be described;* and two or all of them may be used together when wanted. This will indeed be an expense, but not a useless one, since your cooks will work with more comfort, see better than in a smoky kitchen what they are about, your victuals will be

* See Notes at the end of the letter, No. II.

cleaner dressed, and not taste of smoke, as is often the case; and, to render the effect more certain, a stack of three funnels may be safely built higher above the roof than a single funnel.

The case of too short a funnel is more general than would be imagined, and often found where one would not expect it. For it is not uncommon, in ill contrived buildings, instead of having a funnel for each room or fireplace, to bend and turn the funnel of an upper room so as to make it enter the side of another funnel that comes from below. By this means the upper room funnel is made short of course, since its length can only be reckoned from the place where it enters the lower room funnel; and that funnel is also shortened by all the distance between the entrance of the second funnel and the top of the stack; for all that part being readily supplied with air through the second funnel, adds no strength to the draft, especially as that air is cold when there is no fire in the second chimney. The only easy remedy here is, to keep the opening shut of that funnel in which there is no fire.

4. Another very common cause of the smoking of chimneys is *their overpowering one another.* For instance, if there be two chimneys in one large room, and you make fires in both of them, the doors and windows close shut, you will find that the greater and stronger fire shall overpower the weaker, and draw air down its funnel to supply its own demand; which air descending in the weaker funnel, will drive down its smoke, and force it into the room. If, instead of being in one room, the two chimneys are in two different rooms, communicating by a door, the case is the same whenever that door is open. In a very tight house, I have known a kitchen chimney on the lowest floor, when it had a great fire in it, overpower any other chimney

in the house, and draw air and smoke into its room, as often as the door was opened communicating with the staircase.

Remedy. Take care that every room has the means of supplying itself from without, with the air its chimney may require, so that no one of them may be obliged to borrow from another, nor under the necessity of lending. A variety of these means have been already described.

5. Another cause of smoking is, *when the tops of chimneys are commanded by higher buildings, or by a hill,* so that the wind, blowing over such eminences, falls like water over a dam, sometimes almost perpendicularly on the tops of the chimneys that lie in its way, and beats down the smoke contained in them.

Remedy. That commonly applied to this case is a turncap made of tin or plate iron, covering the chimney above and on three sides, open on one side, turning on a spindle, and which, being guided or governed by a vane, always presents its back to the current. This, I believe, may be generally effectual, though not certain, as there may be cases in which it will not succeed. Raising your funnels, if practicable, so as their tops may be higher, or at least equal with the commanding eminence, is more to be depended on. But the turning cap, being easier and cheaper, should first be tried. If obliged to build in such a situation, I would choose to place my doors on the side next the hill, and the backs of my chimneys on the furthest side; for then the column of air falling over the eminence, and of course pressing on that below and forcing it to enter the doors, or *Was-ist-das*es on that side, would tend to balance the pressure down the chimneys, and leave the funnels more free in the exercise of their functions.

6. There is another case of command, the reverse

of that last mentioned. It is where the commanding eminence is farther from the wind than the chimney commanded. To explain this a figure may be necessary. Suppose then a building whose side *A* happens to be exposed to the wind, and forms a kind of dam against its progress. (Plate, Fig. 3.) The air, obstructed by this dam, will, like water, press and search for passages through it; and finding the top of the chimney *B*, below the top of the dam, it will force itself down that funnel, in order to get through by some door or window open on the other side of the building. And if there be a fire in such chimney, its smoke is of course beat down, and fills the room.

Remedy. I know of but one, which is to raise such funnel higher than the roof, supporting it, if necessary, by iron bars. For a turncap in this case has no effect, the dammed up air pressing down through it in whatever position the wind may have placed its opening.

I know a city in which many houses are rendered smoky by this operation. For their kitchens being built behind, and connected by a passage with the houses, and the tops of the kitchen chimneys lower than the top of the houses, the whole side of a street, when the wind blows against its back, forms such a dam as above described; and the wind, so obstructed, forces down those kitchen chimneys (especially when they have but weak fires in them) to pass through the passage and house into the street. Kitchen chimneys, so formed and situated, have another inconvenience. In summer, if you open your upper room windows for air, a light breeze blowing over your kitchen chimney towards the house, though not strong enough to force down its smoke, as aforesaid, is sufficient to waft it into your windows, and fill the rooms with it; which, besides the disagreeableness, damages your furniture.

7. Chimneys, otherwise drawing well, are sometimes made to smoke by *the improper and inconvenient situation of a door.* When the door and chimney are on the same side of the room, as in the figure, if the door *A*, being in the corner, is made to open against the wall, (Plate, Fig. 4,) which is common, as being there, when open, more out of the way, it follows, that, when the door is only opened in part, a current of air rushing in, passes along the wall into and across the opening of the chimney *B*, and flirts some of the smoke out into the room. This happens more certainly when the door is shutting, for then the force of the current is augmented, and becomes very inconvenient to those who, warming themselves by the fire, happen to sit in its way.

The *remedies* are obvious and easy. Either put an intervening screen from the wall round great part of the fireplace; or, which is perhaps preferable, shift the hinges of your door, so as it may open the other way, and, when open, throw the air along the other wall.

8. A room, that has no fire in its chimney, is sometimes filled with *smoke, which is received at the top of its funnel, and descends into the room.* In a former paper* I have already explained the descending currents of air in cold funnels; it may not be amiss, however, to repeat here, that funnels without fires have an effect, according to their degree of coldness or warmth, on the air that happens to be contained in them. The surrounding atmosphere is frequently changing its temperature; but stacks of funnels, covered from winds and sun by the house that contains them, retain a more equal temperature. If, after a warm season, the outward air suddenly grows cold, the empty warm funnels

* See Notes at the end of the letter, No. II.

begin to draw strongly upward; that is, they rarefy the air contained in them, which of course rises, cooler air enters below to supply its place, is rarefied in its turn, and rises; and this operation continues till the funnel grows cooler, or the outward air warmer, or both, when the motion ceases. On the other hand, if after a cold season, the outward air suddenly grows warm and of course lighter, the air contained in the cool funnels, being heavier, descends into the room; and the warmer air which enters their tops, being cooled in its turn and made heavier, continues to descend; and this operation goes on, till the funnels are warmed by the passing of warm air through them, or the air itself grows cooler. When the temperature of the air and of the funnels is nearly equal, the difference of warmth in the air between day and night is sufficient to produce these currents, the air will begin to ascend the funnels as the cool of the evening comes on, and this current will continue till perhaps nine or ten o'clock the next morning, when it begins to hesitate; and as the heat of the day approaches, it sets downwards, and continues so till towards evening, when it again hesitates for some time, and then goes upwards constantly during the night, as before mentioned. Now when smoke issuing from the tops of neighbouring funnels passes over the tops of funnels, which are at the time drawing downwards, as they often are in the middle part of the day, such smoke is of necessity drawn into these funnels, and descends with the air into the chamber.

The *remedy* is to have a sliding plate, hereafter described,* that will shut perfectly the offending funnel.

9. Chimneys, which generally draw well, do nevertheless sometimes give smoke into the rooms, *it being*

* See Notes at the end of the letter, No. II

driven down by strong winds passing over the tops of their funnels, though not descending from any commanding eminence. This case is most frequent where the funnel is short, and the opening turned from the wind. It is the more grievous, when it happens to be a cold wind that produces the effect, because when you most want your fire, you are sometimes obliged to extinguish it. To understand this, it may be considered that the rising light air, to obtain a free issue from the funnel, must push out of its way or oblige the air that is over it to rise. In a time of calm or of little wind this is done visibly, for we see the smoke that is brought up by that air rise in a column above the chimney. But when a violent current of air, that is, a strong wind, passes over the top of a chimney, its particles have received so much force, which keeps them in a horizontal direction, and follow each other so rapidly, that the rising light air has not strength sufficient to oblige them to quit that direction and move upwards to permit its issue. Add to this, that some of the current passing over that side of the funnel which it first meets with, viz. at *A*, (Plate, Fig. 5,) having been compressed by the resistance of the funnel, may expand itself over the flue, and strike the interior opposite side at *B*, from whence it may be reflected downwards and from side to side in the direction of the pricked lines *c c c*.

Remedies. In some places, particularly in Venice, where they have not stacks of chimneys but single flues, the custom is, to open or widen the top of the flue, rounding in the true form of a funnel; (Plate, Fig. 6;) which some think may prevent the effect just mentioned, for that the wind blowing over one of the edges into the funnel, may be slanted out again on the other side by its form. I have had no experience of

this; but I have lived in a windy country, where the contrary is practised, the tops of the flues being *narrowed* inwards, so as to form a slit for the issue of the smoke, long as the breadth of the funnel, and only four inches wide. This seems to have been contrived on a supposition, that the entry of the wind would thereby be obstructed; and perhaps it might have been imagined, that the whole force of the rising warm air being condensed, as it were, in the narrow opening, would thereby be strengthened, so as to overcome the resistance of the wind. This however did not always succeed; for when the wind was at northeast, and blew fresh, the smoke was forced down by fits into the room I commonly sat in, so as to oblige me to shift the fire into another. The position of the slit of this funnel was indeed northeast and southwest. Perhaps if it had lain across the wind, the effect might have been different. But on this I can give no certainty. It seems a matter proper to be referred to experiment. Possibly a turncap might have been serviceable, but it was not tried.

Chimneys have not been long in use in England. I formerly saw a book printed in the time of Queen Elizabeth, which remarked the then modern improvements of living, and mentioned, among others, the convenience of chimneys. "Our forefathers," said the author, "had no chimneys. There was in each dwellinghouse only one place for a fire, and the smoke went out through a hole in the roof; but now there is scarce a gentleman's house in England that has not at least one chimney in it." When there was but one chimney, its top might then be opened as a funnel, and perhaps, borrowing the form from the Venetians, it was then the flue of a chimney got that name. Such is now the growth of luxury, that in both England and France we must have a chimney for every room, and in

some houses every possessor of a chamber, and almost every servant, will have a fire; so that the flues being necessarily built in stacks, the opening of each as a funnel is impracticable. This change of manners soon consumed the firewood of England, and will soon render fuel extremely scarce and dear in France, if the use of coals be not introduced in the latter kingdom as it has been in the former, where it at first met with opposition; for there is extant in the records of one of Queen Elizabeth's parliaments, a motion made by a member, reciting, "That many dyers, brewers, smiths, and other artificers of London, had of late taken to the use of pitcoal for their fires, instead of wood, which filled the air with noxious vapors and smoke, very prejudicial to the health, particularly of persons coming out of the country;" and therefore moving, "that a law might pass to prohibit the use of such fuel (at least during the session of Parliament) by those artificers." It seems it was not then commonly used in private houses. Its supposed unwholesomeness was an objection. Luckily the inhabitants of London have got over that objection, and now think it rather contributes to render their air salubrious, as they have had no general pestilential disorder since the general use of coals, when, before it, such were frequent. Paris still burns wood at an enormous expense continually augmenting, the inhabitants having still that prejudice to overcome. In Germany, you are happy in the use of stoves, which save fuel wonderfully; your people are very ingenious in the management of fire; but they may still learn something in that art from the Chinese,* whose country being greatly populous and fully cultivated, has little room left for the growth of wood, and, having not much other

* See Notes at the end of the letter, No. III.

fuel that is good, they have been forced upon many inventions, during a course of ages, for making a little fire go as far as possible.

I have thus gone through all the common causes of the smoking of chimneys, that I can at present recollect as having fallen under my observation; communicating the remedies that I have known successfully used for the different cases, together with the principles on which both the disease and the remedy depend, and confessing my ignorance wherever I have been sensible of it. You will do well, if you publish, as you propose, this letter, to add in notes, or as you please, such observations as may have occurred to your attentive mind; and, if other philosophers will do the same, this part of science, though humble, yet of great utility, may in time be perfected. For many years past, I have rarely met with a case of a smoky chimney, which has not been solvable on these principles, and cured by these remedies, where people have been willing to apply them; which is indeed not always the case; for many have prejudices in favor of the nostrums of pretending chimney doctors and fumists, and some have conceits and fancies of their own, which they rather chose to try, than to lengthen a funnel, alter the size of an opening, or admit air into a room, however necessary; for some are as much afraid of fresh air as persons in the hydrophobia are of fresh water. I myself had formerly this prejudice, this *aërophobia*, as I now account it; and, dreading the supposed dangerous effects of cool air, I considered it as an enemy, and closed with extreme care every crevice in the rooms I inhabited.

Experience has convinced me of my error. I now look upon fresh air as a friend; I even sleep with an open window. I am persuaded, that no common air from without is so unwholesome, as the air within a

close room, that has been often breathed and not changed. Moist air, too, which formerly I thought pernicious, gives me now no apprehensions; for, considering that no dampness of air applied to the outside of my skin can be equal to what is applied to and touches it within, my whole body being full of moisture, and finding that I can lie two hours in a bath twice a week, covered with water, which certainly is much damper than any air can be, and this for years together, without catching cold, or being in any other manner disordered by it, I no longer dread mere moisture, either in air or in sheets or shirts; and I find it of importance to the happiness of life, the being freed from vain terrors, especially of objects that we are every day exposed inevitably to meet with. You physicians have of late happily discovered, after a contrary opinion had prevailed some ages, that fresh and cool air does good to persons in the smallpox and other fevers. It is to be hoped, that in another century or two we may all find out, that it is not bad even for people in health. And, as to moist air, here I am at this present writing in a ship with above forty persons, who have had no other but moist air to breathe for six weeks past; every thing we touch is damp, and nothing dries, yet we are all as healthy as we should be on the mountains of Switzerland, whose inhabitants are not more so than those of Bermuda or St. Helena, islands on whose rocks the waves are dashed into millions of particles, which fill the air with damp, but produce no diseases, the moisture being pure, unmixed with the poisonous vapors arising from putrid marshes and stagnant pools, in which many insects die and corrupt the water. These places only, in my opinion (which however I submit to yours,) afford unwholesome air; and that it is not the mere water contained in damp air, but the volatile particles of cor-

rupted animal matter mixed with that water, which renders such air pernicious to those who breathe it. And I imagine it a cause of the same kind that renders the air in close rooms, where the perspirable matter is breathed over and over again by a number of assembled people, so hurtful to health. After being in such a situation, many find themselves affected by that *febricula*, which the English alone call *a cold*, and, perhaps from the name, imagine that they caught the malady by *going out* of the room, when it was in fact by being in it.

You begin to think, that I wander from my subject, and go out of my depth. So I return again to my chimneys.

We have of late many lecturers in experimental philosophy. I have wished that some of them would study this branch of that science, and give experiments in it as a part of their lectures. The addition to their present apparatus need not be very expensive. A number of little representations of rooms composed each of five panes of sash glass, framed in wood at the corners, with proportionable doors, and movable glass chimneys, with openings of different sizes, and different lengths of funnel, and some of the rooms so contrived as to communicate on occasion with others, so as to form different combinations, and exemplify different cases; with quantities of green wax taper cut into pieces of an inch and a half, sixteen of which stuck together in a square, and lit, would make a strong fire for a little glass chimney, and blown out would continue to burn and give smoke as long as desired. With such an apparatus all the operations of smoke and rarefied air in rooms and chimneys might be seen through their transparent sides; and the effect of winds on chimneys, commanded or otherwise, might be shown

by letting the entering air blow upon them through an opened window of the lecturer's chamber, where it would be constant while he kept a good fire in his chimney. By the help of such lectures our fumists would become better instructed. At present they have generally but one remedy, which perhaps they have known effectual in some one case of smoky chimneys, and they apply that indiscriminately to all the other cases, without success, — but not without expense to their employers.

With all the science, however, that a man shall suppose himself possessed of in this article, he may sometimes meet with cases that shall puzzle him. I once lodged in a house at London, which, in a little room, had a single chimney and funnel. The opening was very small, yet it did not keep in the smoke, and all attempts to have a fire in this room were fruitless. I could not imagine the reason, till at length, observing that the chamber over it, which had no fireplace in it, was always filled with smoke when a fire was kindled below, and that the smoke came through the cracks and crevices of the wainscot; I had the wainscot taken down, and discovered that the funnel, which went up behind it, had a crack many feet in length, and wide enough to admit my arm, a breach very dangerous with regard to fire, and occasioned probably by an apparent irregular settling of one side of the house. The air entering this breach freely, destroyed the drawing force of the funnel. The remedy would have been, filling up the breach, or rather rebuilding the funnel; but the landlord rather chose to stop up the chimney.

Another puzzling case I met with at a friend's country house near London. His best room had a chimney, in which, he told me, he never could have a fire, for all the smoke came out into the room. I flattered myself

I could easily find the cause, and prescribe the cure. I had a fire made there, and found it as he said. I opened the door, and perceived it was not want of air. I made a temporary contraction of the opening of the chimney, and found that it was not its being too large, that caused the smoke to issue. I went out and looked up at the top of the chimney; its funnel was joined in the same stack with others, some of them shorter, that drew very well, and I saw nothing to prevent its doing the same. In fine, after every other examination I could think of, I was obliged to own the insufficiency of my skill. But my friend, who made no pretension to such kind of knowledge, afterwards discovered the cause himself. He got to the top of the funnel by a ladder, and looking down, found it filled with twigs and straw cemented by earth, and lined with feathers. It seems the house, after being built, had stood empty some years before he occupied it; and he concluded, that some large birds had taken the advantage of its retired situation to make their nest there. The rubbish, considerable in quantity, being removed, and the funnel cleared, the chimney drew well, and gave satisfaction.

In general, smoke is a very tractable thing, easily governed and directed when one knows the principles, and is well informed of the circumstances. You know I made it *descend* in my Pennsylvania stove. I formerly had a more simple construction, in which the same effect was produced, but visible to the eye (Plate, Figure 7.) It was composed of two plates, *A B* and *C D*, placed as in the figure. The lower plate *A B*, rested with its edge in the angle made by the hearth with the back of the chimney. The upper plate was fixed to the breast, and lapped over the lower about six inches, leaving a space of four inches wide and the length of the plates (near two feet) between them. Every other passage of air into the fun-

nel was well stopped. When therefore a fire was made at *E*, for the first time with charcoal, till the air in the funnel was a little heated through the plates, and then wood laid on, the smoke would rise to *A*, turn over the edge of that plate, descend to *D*, then turn under the edge of the upper plate, and go up the chimney. It was pretty to see, but of no great use. Placing therefore the under plate in a higher situation, I removed the upper plate *C D*, and placed it perpendicularly (Plate, Fig. 8), so that the upper edge of the lower plate *A B* came within about three inches of it, and might be pushed further from it, or suffered to come nearer to it, by a movable wedge between them. The flame then ascending from the fire at E, was carried to strike the upper plate, made it very hot, and its heat rose and spread with the rarefied air into the room.

I believe you have seen in use with me the contrivance of a sliding plate over the fire, seemingly placed to oppose the rising of the smoke, leaving but a small passage for it, between the edge of the plate and the back of the chimney. It is particularly described, and its uses explained, in my former printed letter, and I mention it here only as another instance of the tractability of smoke.*

What is called the Staffordshire chimney, (see Plate VI., facing page 50,) affords an example of the same kind. The opening of the chimney is bricked up even with the fore edge of its jambs, leaving open only a passage over the grate of the same width, and perhaps eight inches high. The grate consists of semicircular bars, their upper bar of the greatest diameter, the others under it smaller and smaller, so that it has the appearance of half a round basket. It is, with the coals it

* See Notes at the end of the letter, No. II.

contains, wholly without the wall that shuts up the chimney, yet the smoke bends and enters the passage above it, the draft being strong, because no air can enter that is not obliged to pass near or through the fire, so that all that the funnel is filled with is much heated, and of course much rarefied.

Much more of the prosperity of a winter country depends on the plenty and cheapness of fuel, than is generally imagined. In travelling I have observed, that in those parts where the inhabitants can have neither wood, nor coal, nor turf, but at excessive prices, the working people live in miserable hovels, are ragged, and have nothing comfortable about them. But, when fuel is cheap (or where they have the art of managing it to advantage), they are well furnished with necessaries, and have decent habitations. The obvious reason is, that the working hours of such people are the profitable hours, and they who cannot afford sufficient fuel have fewer such hours in the twenty-four, than those who have it cheap and plenty; for much of the domestic work of poor women, such as spinning, sewing, knitting; and of the men, in those manufactures that require little bodily exercise, cannot well be performed where the fingers are numbed with cold. Those people, therefore, in cold weather, are induced to go to bed sooner, and lie longer in a morning, than they would do, if they could have good fires or warm stoves to sit by; and their hours of work are not sufficient to produce the means of comfortable subsistence. Those public works, therefore, such as roads, canals, &c., by which fuel may be brought cheap into such countries from distant places, are of great utility; and those who promote them may be reckoned among the benefactors of mankind.

I have great pleasure in having thus complied with your request, and in the reflection, that the friendship

you honor me with, and in which I have ever been so happy, has continued so many years without the smallest interruption. Our distance from each other is now augmented, and nature must soon put an end to the possibility of my continuing our correspondence; but, if consciousness and memory remain in a future state, my esteem and respect for you, my dear friend, will be everlasting.

B. FRANKLIN.

Notes to the Letter upon Chimneys.

No. I.

The latest work on architecture, that I have seen, is that entitled *Nutshells*, which appears to be written by a very ingenious man, and contains a table of the proportions of the openings of chimneys; but they relate solely to the proportions he gives his rooms, without the smallest regard to the funnels. And he remarks, respecting those proportions, that they are similar to the harmonic divisions of a monochord.* He does not indeed lay much stress on this; but it shows that we like the appearance of principles; and where we have not true ones, we have some satisfaction in producing such as are imaginary.

No. II.

The description of the sliding plates here promised, and which have been since brought into use under various names, with some immaterial changes, is contained in a former letter to James Bowdoin, as follows,

* "It may be just remarked here, that upon comparing these proportions with those arising from the common divisions of the monochord, it happens, that the first answers to unisons; and, although the second is a discord, the third answers to the third minor, the fourth to the third major, the fifth to the fourth, the sixth to the fifth, and the seventh to the octave." —*Nutshells*, page 85.

London, 2 December, 1758.

DEAR SIR,

I have executed here an easy, simple contrivance, that I have long since had in speculation, for keeping rooms warmer in cold weather than they generally are, and with less fire. It is this. The opening of the chimney is contracted, by brick work faced with marble slabs, to about two feet between the jambs, and the breast brought down to within about three feet of the hearth. An iron frame is placed just under the breast, and extending quite to the back of the chimney, so that a plate of the same metal may slide horizontally backwards and forwards in the grooves on each side of the frame. This plate is just so large as to fill the whole space, and shut the chimney entirely when thrust quite in, which is convenient when there is no fire. Drawing it out, so as to leave a space between its further edge and the back, of about two inches; this space is sufficient for the smoke to pass; and so large a part of the funnel being stopped by the rest of the plate, the passage of warm air out of the room, up the chimney, is obstructed and retarded, and by that means much cold air is prevented from coming in through crevices, to supply its place.

This effect is made manifest three ways. First, when the fire burns briskly in cold weather, the howling or whistling noise made by the wind, as it enters the room through the crevices, when the chimney is open as usual, ceases as soon as the plate is slid in to its proper distance. Secondly, opening the door of the room about half an inch, and holding your hand against the opening, near the top of the door, you feel the cold air coming in against your hand, but weakly, if the plate be in. Let another person suddenly draw it out,

so as to let the air of the room go up the chimney, with its usual freedom where chimneys are open, and you immediately feel the cold air rushing in strongly. Thirdly, if something be set against the door, just sufficient, when the plate is in, to keep the door nearly shut, by resisting the pressure of the air that would force it open; then, when the plate is drawn out, the door will be forced open by the increased pressure of the outward cold air endeavouring to get in to supply the place of the warm air, that now passes out of the room to go up the chimney. In our common open chimneys, half the fuel is wasted, and its effect lost; the air it has warmed being immediately drawn off. Several of my acquaintance, having seen this simple machine in my room, have imitated it at their own houses, and it seems likely to become pretty common. I describe it thus particularly to you, because I think it would be useful in Boston, where firing is often dear.

Mentioning chimneys puts me in mind of a property I formerly had occasion to observe in them, which I have not found taken notice of by others; it is, that in the summer time, when no fire is made in the chimneys, there is, nevertheless, a regular draft of air through them, continually passing upwards, from about five or six o'clock in the afternoon, till eight or nine o'clock the next morning, when the current begins to slacken and hesitate a little for about half an hour, and then sets as strongly down again, which it continues to do till towards five in the afternoon, then slackens and hesitates as before, going sometimes a little up, then a little down, till, in about a half an hour, it gets into a steady upward current for the night, which continues till eight or nine the next day; the hours varying a little as the days lengthen and shorten, and sometimes varying from sudden changes in the weather; as, if,

after being long warm, it should begin to grow cool about noon, while the air was coming down the chimney, the current will then change earlier than the usual hour, &c.

This property in chimneys I imagine we might turn to some account, and render improper, for the future, the old saying, *as useless as a chimney in summer.* If the opening of the chimney, from the breast down to the hearth, be closed by a slight movable frame or two, in the manner of doors covered with canvass, that will let the air through, but keep out the flies; and another little frame set within upon the hearth, with hooks on which to hang joints of meat, fowls, &c. wrapped well in wet linen cloths, three or four fold, I am confident, that, if the linen is kept wet by sprinkling it once a day, the meat would be so cooled by the evaporation, carried on continually by means of the passing air, that it would keep a week or more in the hottest weather. Butter and milk might likewise be kept cool, in vessels or bottles covered with wet cloths. A shallow tray, or keeler, should be under the frame to receive any water that might drip from the wetted cloths. I think, too, that this property of chimneys might, by means of smokejack vanes, be applied to some mechanical purposes, where a small but pretty constant power only is wanted.

If you would have my opinion of the cause of this changing current of air in chimneys, it is, in short, as follows. In summer time there is generally a great difference in the warmth of the air at midday and midnight, and, of course, a difference of specific gravity in the air, as, the more it is warmed, the more it is rarefied. The funnel of a chimney, being for the most part surrounded by the house, is protected, in a great measure, from the direct action of the sun's rays, and also

from the coldness of the night air. It thence preserves a middle temperature between the heat of the day, and the coldness of the night. This middle temperature it communicates to the air contained in it. If the state of the outward air be cooler than that in the funnel of the chimney, it will, by being heavier, force it to rise, and go out at the top. What supplies its place from below, being warmed, in its turn, by the warmer funnel, is likewise forced up by the colder and weightier air below, and so the current is continued till the next day, when the sun gradually changes the state of the outward air, makes it first as warm as the funnel of the chimney can make it (when the current begins to hesitate) and afterwards warmer. Then the funnel, being cooler than the air that comes into it, cools that air, makes it heavier than the outward air, of course it descends; and what succeeds it from above being cooled in its turn, the descending current continues till towards evening, when it again hesitates and changes its course, from the change of warmth in the outward air, and the nearly remaining same middle temperature in the funnel.

Upon this principle, if a house were built behind Beacon Hill, an adit carried from one of the doors into the hill horizontally, till it meet with a perpendicular shaft sunk from its top, it seems probable to me, that those who lived in the house would constantly, in the heat even of the calmest day, have as much cool air passing through the house, as they should choose; and the same, though reversed in its current, during the stillest night.

I think, too, this property might be made of use to miners; as, where several shafts or pits are sunk perpendicularly into the earth, communicating at bottom by horizontal passages, which is a common case, if a chimney of thirty or forty feet high were built over one

of the shafts, or so near the shaft, that the chimney might communicate with the top of the shaft, all air being excluded but what should pass up or down by the shaft, a constant change of air would, by this means, be produced in the passages below, tending to secure the workmen from those damps, which so frequently incommode them. For the fresh air would be almost always going down the open shaft, to go up the chimney, or down the chimney, to go up the shaft. Let me add one observation more, which is, that, if that part of the funnel of a chimney, which appears above the roof of a house, be pretty long, and have three of its sides exposed to the heat of the sun successively, viz. when he is in the east, in the south, and in the west, while the north side is sheltered by the building from the cool northerly winds; such a chimney will often be so heated by the sun, as to continue the draft strongly upwards, through the whole twenty-four hours, and often for many days together. If the outside of such a chimney be painted black, the effect will be still greater, and the current stronger.

No. III.

It is said the northern Chinese have a method of warming their ground floors, which is ingenious. Those floors are made of tiles, a foot square and two inches thick, their corners being supported by bricks set on end, that are a foot long and four inches square; the tiles, too, join into each other, by ridges and hollows along their sides. This forms a hollow under the whole floor, which on one side of the house has an opening into the air, where a fire is made, and it has a funnel rising from the other side to carry off the smoke. The fuel is a sulphurous pitcoal, the smell of which in the room is thus avoided, while the floor, and of course the

room is well warmed. But, as the underside of the floor must grow foul with soot, and a thick coat of soot prevents much of the direct application of the hot air to the tiles, I conceive that burning the smoke, by obliging it to descend through red coals, would in this construction be very advantageous, as more heat would be given by the flame than by the smoke, and the floor being thereby kept free from soot would be more heated with less fire. For this purpose I would propose erecting the funnel close to the grate, so as to have only an iron plate between the fire and the funnel, through which plate, the air in the funnel being heated, it will be sure to draw well, and force the smoke to descend, as in Plate XIV. Fig. 9, where *A* is the funnel or chimney, *B* the grate on which the fire is placed, *C* one of the apertures through which the descending smoke is drawn into the channel *D* of figure 10, along which channel it is conveyed by a circuitous route, as designated by the arrows, until it arrives at the small aperture *E*, through which it enters the funnel *F*. *G*, in both figures, is the iron plate against which the fire is made, which, being heated thereby, will rarefy the air in that part of the funnel, and cause the smoke to ascend rapidly. The flame thus dividing from the grate to the right and left, and turning in passages, disposed, as in figure 10, so as that every part of the floor may be visited by it before it enters the funnel *F*, by the two passages *E E*, very little of the heat will be lost, and a winter room thus rendered very comfortable.

No. IV.

Page 513. *Few can imagine*, &c. It is said the Icelanders have very little fuel, chiefly drift wood that comes upon their coast. To receive more advantage from its heat, they make their doors low, and have a

stage round the room above the door, like a gallery, wherein the women can sit and work, the men read or write, &c. The roof being tight, the warm air is confined by it and kept from rising higher and escaping; and the cold air, which enters the house when the door is opened, cannot rise above the level of the top of the door, because it is heavier than the warm air above the door, and so those in the gallery are not incommoded by it. Some of our too lofty rooms might have a stage so constructed as to make a temporary gallery above, for the winter, to be taken away in summer. Sedentary people would find much comfort there in cold weather.

No. V.

Page 532. *Where they have the art of managing it*, &c. In some houses of the lower people among the northern nations of Europe, and among the poorer sort of Germans in Pennsylvania, I have observed this construction, which appears very advantageous. (Plate, Fig. 11.) *A* is the kitchen with its chimney; *B*, an iron stove in the stove room. In a corner of the chimney is a hole through the back into the stove, to put in fuel, and another hole above it to let the smoke of the stove come back into the chimney. As soon as the cooking is over, the brands in the kitchen chimney are put through the hole to supply the stove, so that there is seldom more than one fire burning at a time. In the floor over the stove room is a small trap door, to let the warm air rise occasionally into the chamber. Thus the whole house is warmed at little expense of wood, and the stove room kept constantly warm; so that in the coldest winter nights, they can work late, and find the room still comfortable when they rise to work early. An English farmer in America, who makes great fires

in large open chimneys, needs the constant employment of one man to cut and haul wood for supplying them; and the draft of cold air to them is so strong, that the heels of his family are frozen, while they are scorching their faces, and the room is never warm, so that little sedentary work can be done by them in winter. The difference in this article alone of economy shall, in a course of years, enable the German to buy out the Englishman, and take possession of his plantation.

Miscellaneous Observations.

Chimneys, whose funnels go up in the north wall of a house, and are exposed to the north winds, are not so apt to draw well as those in a south wall; because, when rendered cold by those winds, they draw downwards.

Chimneys, enclosed in the body of a house, are better than those whose funnels are exposed in cold walls.

Chimneys in stacks are apt to draw better than separate funnels, because the funnels, that have constant fires in them, warm the others, in some degree, that have none.

One of the funnels, in a house I once occupied, had a particular funnel joined to the south side of the stack, so that three of its sides were exposed to the sun in the course of the day, viz. (Plate, Fig. 12,) the east side *E* during the morning, the south side *S* in the middle part of the day, and the west side *W* during the afternoon, while its north side was sheltered by the stack from the cold winds. This funnel, which came from the ground floor, and had a considerable height above the roof, was constantly in a strong drawing state day and night, winter and summer.

Blacking of funnels, exposed to the sun, would probably make them draw still stronger.

In Paris I saw a fireplace so ingeniously contrived as to serve conveniently two rooms, a bedchamber and a study. The funnel over the fire was round. The fireplace was of cast iron (Plate, Fig. 13,) having an upright back *A*, and two horizontal semicircular plates *B C*, the whole so ordered as to turn on the pivots *D E*. The plate *B* always stopped that part of the round funnel that was next to the room without fire, while the other half of the funnel over the fire was always open. By this means a servant in the morning could make a fire on the hearth *C*, then in the study, without disturbing the master by going into his chamber; and the master, when he rose, could, with a touch of his foot, turn the chimney on its pivots, and bring the fire into his chamber, keep it there as long as he wanted it, and turn it again, when he went out, into his study. The room which had no fire in it was also warmed by the heat coming through the back plate, and spreading in the room, as it could not go up the chimney.

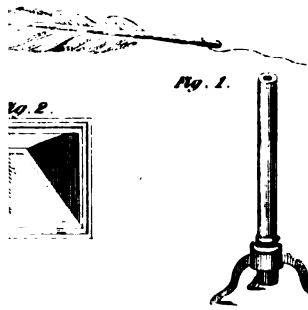


Fig. 1.

Fig. 2.

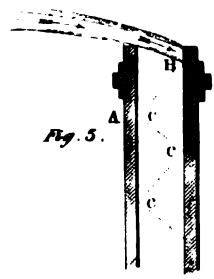


Fig. 3.

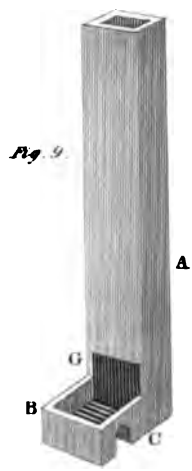


Fig. 4.

Description of a new Stove for burning Pitcoal, and consuming all its Smoke.

READ AT A MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY, JANUARY 28TH, 1786.

TOWARDS the end of the last century an ingenious French philosopher, whose name I am sorry I cannot recollect, exhibited an experiment to show, that very offensive things might be burnt in the middle of a chamber, such as woollen rags, feathers, &c., without creating the least smoke or smell. The machine in which it was made, if I remember right, was of this form, (Plate XV. Fig. 1,) made of plate iron. Some clear burning charcoals were put into the opening of the short tube *A*, and supported there by the grate *B*. The air, as soon as the tubes grew warm, would ascend in the longer leg *C* and go out at *D*, consequently air must enter at *A* descending to *B*. In this course it must be heated by the burning coals through which it passed, and rise more forcibly in the longer tube, in proportion to its degree of heat or rarefaction, and length of that tube. For such a machine is a kind of inverted siphon; and, as the greater weight of water in the longer leg of a common siphon in descending is accompanied by an ascent of the same fluid in the shorter; so, in this inverted siphon, the greater quantity of levity of air in the longer leg, in rising is accompanied by the descent of air in the shorter. The things to be burned being laid on the hot coals at *A*, the smoke must descend through those coals, and be converted into flame, which, after destroying the offensive smell, came out at the end of the longer tube as mere heated air.

Whoever would repeat this experiment with success, must take care that the part *A B*, of the short tube, be

quite full of burning coals, so that no part of the smoke may descend and pass by them without going through them, and being converted into flame; and that the longer tube be so heated as that the current of ascending hot air is established in it before the things to be burnt are laid on the coals; otherwise there will be a disappointment.

It does not appear, either in the Memoirs of the Academy of Sciences, or Philosophical Transactions of the English Royal Society, that any improvement was ever made of this ingenious experiment, by applying it to useful purposes. But there is a German book, entitled *Vulcanus Famulans*, by John George Leutmann, P. D., printed at Wirtemberg in 1723, which describes, among a great variety of other stoves for warming rooms, one, which seems to have been formed on the same principle, and probably from the hint thereby given, though the French experiment is not mentioned. This book being scarce, I have translated the chapter describing the stove, viz.

"CHAPTER VII.

"*On a Stove, which draws downwards.*

"Here follows the description of a sort of stove, which can easily be removed, and again replaced at pleasure. This drives the fire down under itself, and gives no smoke, but, however, a very unwholesome vapor.

"In the figure, *A* is an iron vessel like a funnel, (Plate XV. Fig. 20,) in diameter at the top about twelve inches, at the bottom near the grate about five inches; its height twelve inches. This is set on the barrel *C*, which is ten inches diameter and two feet long, closed at each end *E E*. From one end rises a

pipe or flue about four inches diameter, on which other pieces of pipe are set, which are gradually contracted to *D*, where the opening is but about two inches. Those pipes must together be at least four feet high. *B* is an iron grate. *FF* are iron handles guarded with wood, by which the stove is to be lifted and moved. It stands on three legs. Care must be taken to stop well all the joints, that no smoke may leak through.

"When this stove is to be used, it must first be carried into the kitchen and placed in the chimney near the fire. There burning wood must be laid and left upon its grate till the barrel *C* is warm, and the smoke no longer rises at *A*, but descends towards *C*. Then it is to be carried into the room which it is to warm. When once the barrel *C* is warm, fresh wood may be thrown into the vessel *A* as often as one pleases, the flame descends and without smoke, which is so consumed that only a vapor passes out at *D*.

"As this vapor is unwholesome, and affects the head, one may be freed from it, by fixing in the wall of the room an inverted funnel, such as people use to hang over lamps, through which their smoke goes out as through a chimney. This funnel carries out all the vapor cleverly, so that one finds no inconvenience from it, even though the opening *D* be placed a span below the mouth of the said funnel *G*. The neck of the funnel is better when made gradually bending, than if turned in a right angle.

"The cause of the draft downwards in the stove is the pressure of the outward air, which, falling into the vessel *A* in a column of twelve inches diameter, finds only a resisting passage at the grate *B*, of five inches, and one at *D*, of two inches, which are much too weak to drive it back again; besides, *A* stands much higher than *B*, and so the pressure on it is greater and more

forcible, and beats down the flame to that part where it finds the least resistance. Carrying the machine first to the kitchen fire for preparation is on this account, that in the beginning the fire and smoke naturally ascend, till the air in the close barrel *C* is made thinner by the warmth. When that vessel is heated, the air in it is rarefied, and then all the smoke and fire descends under it.

"The wood should be thoroughly dry, and cut into pieces five or six inches long, to fit it for being thrown into the funnel *A*."

It appears to me, by Mr. Leutmann's explanation of the operation of this machine, that he did not understand the principles of it, whence I conclude he was not the inventor of it; and by the description of it, wherein the opening at *A* is made so large, and the pipe *E D*, so short, I am persuaded he never made nor saw the experiment, for the first ought to be much smaller, and the last much higher, or it hardly will succeed. The carrying it in the kitchen, too, every time the fire should happen to be out, must be so troublesome, that it is not likely ever to have been in practice, and probably has never been shown but as a philosophical experiment. The funnel for conveying the vapor out of the room would besides have been uncertain in its operation, as a wind blowing against its mouth would drive the vapor back.

The stove I am about to describe was also formed on the idea given by the French experiment, and completely carried into execution before I had any knowledge of the German invention; which I wonder should remain so many years in a country, where men are so ingenious in the management of fire, without receiving long since the improvements I have given it.

Description of the Parts.

A, the bottom plate which lies flat upon the hearth, with its partitions, 1, 2, 3, 4, 5, 6, (Plate, Fig. 2,) that are cast with it, and a groove *Z Z*, in which are to slide the bottom edges of the small plates *Y*, *Y*, figure 12; which plates meeting at *X* close the front.

B 1, figure 3, is the cover plate showing its under side, with the grooves 1, 2, 3, 4, 5, 6, to receive the top edges of the partitions that are fixed to the bottom plate. It shows also the grate *W W*, the bars of which are cast in the plate, and a groove *V V*, which comes right over the groove *Z Z*, figure 2, receiving the upper edges of the small sliding plates *Y*, *Y*, figure 12.

B 2, figure 4, shows the upper side of the same plate, with a square impression or groove for receiving the bottom mouldings *T T T T* of the three-sided box *C*, figure 5, which is cast in one piece.

D, figure 6, its cover, showing its under side with grooves to receive the upper edges *S S S* of the sides of *C*, figure 5, also a groove *R R*, which, when the cover is put on, comes right over another *Q Q* in *C*, figure 5, between which is to slide

E, figure 7, the front plate of the box.

P, a hole three inches diameter through the cover *D*, figure 6, over which hole stands the vase *F*, figure 8, which has a corresponding hole two inches diameter, through its bottom.

The top of the vase opens at *O O O*, figure 8, and turns back upon a hinge behind, when coals are to be put in; the vase has a grate within at *N N* of cast iron *H*, figure 9, and a hole in the top, one and a half inches diameter, to admit air, and to receive the ornamental orass gilt flame *M*, figure 10, which stands in that

hole, and, being itself hollow and open, suffers air to pass through it to the fire.

G, figure 11, is a drawer of plate iron, that slips in between in the partitions 2 and 3, figure 2, to receive the falling ashes. It is concealed when the small sliding plates *Y*, *Y*, figure 12, are shut together.

I I I I, figure 8, is a niche built of brick in the chimney, and plastered. It closes the chimney over the vase, but leaves two funnels, one in each corner, communicating with the bottom box *K K*, figure 2.

Dimensions of the Parts.

	Feet.	In.
Front of the bottom box, - - - - - -	2	0
Height of its partitions, - - - - - -	0	4¼
Length of No. 1, 2, 3 and 4, each, -	1	3
Length of No. 5 and 6, each, - - -	0	8¼
Breadth of the passage between No. 2 and 3, - - - - - - - - - - - -	0	6
Breadth of the other passages each, -	0	3½
Breadth of the grate, - - - - - - -	0	6½
Length of ditto, - - - - - - - - -	0	8
Bottom moulding of box *C*, square, - -	1	0
Height of the sides of ditto, - - - - -	0	4
Length of the back side, - - - - - -	0	10
Length of the right and left sides, each,	0	9½
Length of the front plate *E*, where longest,	0	11
The cover *D*, square, - - - - - - -	0	12
Hole in ditto, diameter, - - - - - -	0	3
Sliding plates *Y*, *Y*, their length, each, -	1	0
——————— their breadth, each, -	0	4½
Drawer *G*, its length, - - - - - - -	1	0
——————— breadth, - - - - - -	0	5¾
——————— depth, - - - - - - -	0	4

Drawer *G*, depth of its further end, only,	0	1
Grate *H* in the vase, its diameter to the extremity of its knobs, - - - - - -	0	5¾
Thickness of the bars at top, - - - - -	0	0½
——————— at bottom, less, -	0	0
Depth of the bars at the top, - - - -	0	0¾
Height of the vase, - - - - - - - -	1	6
Diameter of the opening *O O*, in the clear,	0	8
Diameter of the air hole at top, - - -	0	1½
——— of the flame hole at bottom, -	0	2

To fix this Machine.

Spread mortar on the hearth to bed the bottom plate *A*, then lay that plate level, equally distant from each jamb, and projecting out as far as you think proper. Then putting some Windsor loam in the grooves of the cover *B*, lay that on; trying the sliding plates *Y*, *Y*, to see if they move freely in the grooves *Z Z*, *V V*, designed for them.

Then begin to build the niche, observing to leave the square corners of the chimney unfilled; for they are to be funnels. And observe also to leave a free open communication between the passages at *K*, *K*, and the bottom of those funnels, and mind to close the chimney above the top of the niche, that no air may pass up that way. The concave back of the niche will rest on the circular iron partition 1 *A* 4, figure 2, then, with a little loam, put on the box *C* over the grate, the open side of the box in front.

Then, with loam in three of its grooves, the groove *R R* being left clean, and brought directly over the groove *Q Q* in the box, put on the cover *D*, trying the front plate *E*, to see if it slides freely in those grooves.

Lastly, set on the vase, which has small holes in the moulding of its bottom to receive two iron pins that rise out of the plate *D* at *I, I,* for the better keeping it steady.

Then putting in the grate *H,* which rests on its three knobs *h h h* against the inside of the vase, and slipping the drawer into its place; the machine is fit for use.

To use it.

Let the first fire be made after eight in the evening, or before eight in the morning, for at those times and between those hours all night, there is usually a draft up a chimney, though it has long been without fire; but between those hours in the day there is often, in a cold chimney, a draft downwards, when, if you attempt to kindle a fire, the smoke will come into the room.

But to be certain of your proper time, hold a flame over the air hole at the top. If the flame is drawn strongly down for a continuance, without whiffling, you may begin to kindle a fire.

First put in a few charcoals on the grate *H.*

Lay some small sticks on the charcoals.

Lay some pieces of paper on the sticks.

Kindle the paper with a candle.

Then shut down the top, and the air will pass down through the air hole, blow the flame of the paper down through the sticks, kindle them, and their flame passing lower kindles the charcoal.

When the charcoal is well kindled, lay on it the sea-coals, observing not to choak the fire by putting on too much at first.

The flame descending through the hole in the bottom of the vase, and that in plate *D,* into the box *C,* passes down farther through the grate *W W* in plate *B* 1,

then passes horizontally towards the back of the chimney; there dividing, and turning to the right and left, one part of it passes round the far end of the partition 2, then coming forward it turns round the near end of partition 1, then moving backward it arrives at the opening into the bottom of one of the upright corner funnels behind the niche, through which it ascends into the chimney, thus heating that half of the box and that side of the niche. The other part of the divided flame passes round the far end of partition 3, round the near end of partition 4, and so into and up the other corner funnel, thus heating the other half of the box, and the other side of the niche. The vase itself, and the box *C*, will also be very hot, and the air surrounding them being heated, and rising, as it cannot get into the chimney, it spreads in the room, colder air succeeding is warmed in its turn, rises and spreads, till by the continual circulation the whole is warmed.

If you should have occasion to make your first fire at hours not so convenient as those above mentioned, and when the chimney does not draw, do not begin it in the vase, but in one or more of the passages of the lower plate, first covering the mouth of the vase. After the chimney has drawn a while with the fire thus low, and begins to be a little warm, you may close those passages and kindle another fire in the box *C*, leaving its sliding shutter a little open; and when you find, after some time, that the chimney, being warmed, draws forcibly, you may shut that passage, open your vase, and kindle your fire there, as above directed. The chimney well warmed by the first day's fire will continue to draw constantly all winter, if fires are made daily.

You will, in the management of your fire, have need of the following implements;

A pair of small light tongs, twelve or fifteen inches long; Plate, figure 13.

A light poker about the same length, with a flat broad point, figure 14.

A rake to draw ashes out of the passages of the lower plate, where the lighter kind escaping the ash-box will gather by degrees, and perhaps once in a week or ten days require being removed, figure 15.

And a fork with its prongs wide enough to slip on the neck of the vase cover, in order to raise and open it when hot, to put in fresh coals, figure 16.

In the management of this stove, there are certain precautions to be observed at first, with attention, till they become habitual. To avoid the inconvenience of smoke, see that the grate *H* be clear before you begin to light a fresh fire. If you find it clogged with cinders and ashes, turn it up with your tongs and let them fall upon the grate below; the ashes will go through it, and the cinders may be raked off and returned into the vase when you would burn them. Then see that all the sliding plates are in their places and close shut, that no air may enter the stove but through the round opening at the top of the vase. And to avoid the inconvenience of dust from the ashes, let the ash-drawer be taken out of the room to be emptied; and, when you rake the passages, do it when the draft of the air is strong inwards, and put the ashes carefully into the ash-box, that remaining in its place.

If, being about to go abroad, you would prevent your fire burning in your absence, you may do it by taking the brass flame from the top of the vase, and covering the passage with a round tin plate, which will prevent the entry of more air than barely sufficient to keep a few of the coals alive. When you return, though some hours absent, by taking off the tin plate and admitting the air, your fire will soon be recovered.

The effect of this machine, well managed, is to burn not only the coals, but all the smoke of the coals, so that while the fire is burning, if you go out and observe the top of your chimney, you will see no smoke issuing, nor any thing but clear warm air, which as usual makes the bodies seen through it appear waving.

But let none imagine from this, that it may be a cure for bad or smoky chimneys, much less, that, as it burns the smoke, it may be used in a room that has no chimney. It is by the help of a good chimney, the higher the better, that it produces its effect; and, though a flue of plate iron sufficiently high might be raised in a very lofty room, the management to prevent all disagreeable vapor would be too nice for common practice, and small errors would have unpleasing consequences.

It is certain, that clean iron yields no offensive smell when heated. Whatever of that kind you perceive, where there are iron stoves, proceeds therefore from some foulness burning or fuming on their surface. They should therefore never be spit upon, or greased, nor should any dust be suffered to lie upon them. But, as the greatest care will not always prevent these things, it is well once a week to wash the stove with soap lees and a brush, rinsing it with clean water.

The Advantages of this Stove.

1. The chimney does not grow foul, nor ever need sweeping; for as no smoke enters it, no soot can form in it.

2. The air heated over common fires instantly quits the room and goes up the chimney with the smoke; but in the stove, it is obliged to descend in flame and pass through the long winding horizontal passages, communicating its heat to a body of iron plate, which, having thus time to receive the heat, communicates

the same to the air of the room, and thereby warms it to a greater degree.

3. The whole of the fuel is consumed by being turned into flame, and you have the benefit of its heat; whereas, in common chimneys, a great part goes away in smoke which you see as it rises, but it affords you no rays of warmth. One may obtain some notion of the quantity of fuel thus wasted in smoke, by reflecting on the quantity of soot that a few weeks firing will lodge against the sides of the chimney, and yet this is formed only of those particles of the column of smoke that happen to touch the sides in its ascent. How much more must have passed off in the air? And we know that this soot is still fuel; for it will burn and flame as such, and when hard caked together is indeed very like and almost as solid as the coal it proceeds from. The destruction of your fuel goes on nearly in the same quantity, whether in smoke or in flame; but there is no comparison in the difference of heat given. Observe when fresh coals are first put on your fire, what a body of smoke arises. This smoke is for a long time too cold to take flame. If you then plunge a burning candle into it, the candle, instead of inflaming the smoke, will instantly be itself extinguished. Smoke must have a certain degree of heat to be inflammable. As soon as it has acquired that degree, the approach of a candle will inflame the whole body, and you will be very sensible of the difference of the heat it gives. A still easier experiment may be made with the candle itself. Hold your hand near the side of its flame, and observe the heat it gives; then blow it out, the hand remaining in the same place, and observe what heat may be given by the smoke that rises from the still burning snuff. You will find it very little. And yet that smoke has in it the substance of so

much flame, and will instantly produce it, if you hold another candle above it so as to kindle it. Now the smoke from the fresh coals laid on this stove, instead of ascending and leaving the fire while too cold to burn, being obliged to descend through the burning coals, receives among them that degree of heat which converts it into flame, and the heat of that flame is communicated to the air of the room, as above explained.

4. The flame from the fresh coals laid on in this stove, descending through the coals already ignited, preserves them long from consuming, and continues them in the state of red coals as long as the flame continues that surrounds them, by which means the fires made in this stove are of much longer duration than in any other, and fewer coals are therefore necessary for a day. This is a very material advantage indeed. That flame should be a kind of pickle, to preserve burning coals from consuming, may seem a paradox to many, and very unlikely to be true, as it appeared to me the first time I observed the fact. I must therefore relate the circumstances, and shall mention an easy experiment, by which my reader may be in possession of every thing necessary to the understanding of it. In the first trial I made of this kind of stove, which was constructed of thin plate iron, I had, instead of the vase, a kind of inverted pyramid like a mill-hopper; and fearing at first that the small grate contained in it might be clogged by cinders, and the passage of the flame sometimes obstructed, I ordered a little door near the grate, by means of which I might on occasion clear it; though, after the stove was made and before I tried it, I began to think this precaution superfluous, from an imagination, that the flame being contracted in the narrow part where the grate

was placed, would be more powerful in consuming what it should there meet with, and that any cinders between or near the bars would be presently destroyed and the passage opened.

After the stove was fixed and in action, I had a pleasure now and then in opening that door a little, to see through the crevice how the flame descended among the red coals; and, observing once a single coal lodged on the bars in the middle of the focus, a fancy took me to observe by my watch in how short a time it would be consumed. I looked at it long without perceiving it to be at all diminished, which surprised me greatly. At length it occurred to me, that I and many others had seen the same thing thousands of times, in the conservation of the red coal formed in the snuff of a burning candle, which, while enveloped in flame, and thereby prevented from the contact of passing air, is long continued and augments instead of diminishing, so that we are often obliged to remove it by the snuffers, or bend it out of the flame into the air, where it consumes presently to ashes. I then supposed, that to consume a body by fire, passing air was necessary to receive and carry off the separated particles of the body; and that the air passing in the flame of my stove, and in the flame of a candle, being already saturated with such particles, could not receive more, and therefore left the coal undiminished as long as the outward air was prevented from coming to it by the surrounding flame, which kept it in a situation somewhat like that of charcoal in a well luted crucible, which, though long kept in a strong fire, comes out unconsumed.

An easy experiment will satisfy any one of this conserving power of flame enveloping red coal. Take a small stick of deal or other wood the size of a goose quill, and hold it horizontally and steadily in the flame

of the candle above the wick, without touching it, but in the body of the flame. The wood will first be inflamed, and burn beyond the edge of the flame of the candle, perhaps a quarter of an inch. When the flame of the wood goes out, it will leave a red coal at the end of the stick, part of which will be in the flame of the candle and part out in the air. In a minute or two you will perceive the coal in the air diminish gradually, so as to form a neck; while the part in the flame continues of its first size, and at length the neck being quite consumed, it drops off; and, by rolling it between your fingers when extinguished, you will find it still a solid coal.

However, as one cannot be always putting on fresh fuel in this stove to furnish a continual flame, as is done in a candle, the air in the intervals of time gets at the red coals and consumes them. Yet the conservation while it lasted, so much delayed the consumption of the coals, that two fires, one made in the morning, and the other in the afternoon, each made by only a hatfull of coals, were sufficient to keep my writing room, about sixteen feet square and ten high, warm a whole day. The fire kindled at seven in the morning would burn till noon; and, all the iron of the machine with the walls of the niche being thereby heated the room kept warm till evening, when another smaller fire kindled kept it warm till midnight.

Instead of the sliding plate *E*, which shuts the front of the box *C*, I sometimes used another, which had a pane of glass, or, which is better, of Muscovy talc, that the flame might be seen descending from the bottom of the vase and passing in a column through the box *C*, into the cavities of the bottom plate, like water falling from a funnel, admirable to such as are not acquainted with the nature of the machine, and in itself a pleasing spectacle.

Every utensil, however properly contrived to serve its purpose, requires some practice before it can be used adroitly. Put into the hands of a man for the first time a gimblet or a hammer, (very simple instruments,) and tell him the use of them, he shall neither bore a hole or drive a nail with the dexterity and success of another, who has been accustomed to handle them. The beginner, therefore, in the use of this machine, will do well not to be discouraged with little accidents, that may arise at first from his want of experience. Being somewhat complex, it requires, as already said, a variety of attentions; habit will render them unnecessary. And the studious man, who is much in his chamber, and has a pleasure in managing his own fire, will soon find this a machine most comfortable and delightful. To others, who leave their fires to the care of ignorant servants, I do not recommend it. They will with difficulty acquire the knowledge necessary, and will make frequent blunders, that will fill your room with smoke. It is therefore by no means fit for common use in families. It may be advisable to begin with the flaming kind of stone coal, which is large, and, not caking together, is not so apt to clog the grate. After some experience, any kind of coal may be used, and with this advantage, that no smell, even from the most sulphurous kind, can come into your room, the current of air being constantly into the vase, where too that smell is all consumed.

The vase form was chosen as being elegant in itself, and very proper for burning of coals. Where wood is the usual fuel, and must be burned in pieces of some length, a long square chest may be substituted, in which *A* is the cover opening by a hinge behind, *B* the grate, *C* the hearth-box with its divisions as in the other, *D* the plan of the chest, *E* the long narrow grate. Plate,

Fig. 17. This I have not tried, but the vase machine was completed in 1771, and used by me in London three winters, and one afterwards in America, much to my satisfaction; and I have not yet thought of any improvement it may be capable of, though such may occur to others. For common use, while in France, I have contrived another grate for coals, which has in part the same property of burning the smoke and preserving the red coals longer by the flame, though not so completely as in the vase, yet sufficiently to be very useful, which I shall now describe as follows.

A, is a round grate, one French foot in diameter, and eight inches deep between the bars and the back (Plate, Fig. 18); the sides and back of plate iron; the sides having holes of half an inch diameter, distant three or four inches from each other, to let in air for enlivening the fire. The back without holes. The sides do not meet at top nor at bottom by eight inches; that space is filled by grates of small bars crossing front to back to let in air below, and let out the smoke or flame above. The three middle bars of the front grate are fixed, the upper and lower may be taken out and put in at pleasure, when hot, with a pair of pincers. This round grate turns upon an axis, supported by the crotchet *B*, the stem of which is an inverted conical tube five inches deep, which comes on as many inches upon a pin that fits it, and which is fixed upright in a cast-iron plate *D*, that lies upon the hearth; in the middle of the top and bottom grates are fixed small upright pieces, *E*, *E*, about an inch high, which, as the whole is turned on its axis, stop it when the grate is perpendicular. Fig. 19 is another view of the same machine.

In making the first fire in a morning with this grate, there is nothing particular to be observed. It is made as in other grates, the coals being put in above, after

taking out the upper bar, and replacing it when they are in. The round figure of the fire, when thoroughly kindled, is agreeable. It represents the Great Giver of warmth to our system. As it burns down and leaves a vacancy above, which you would fill with fresh coals, the upper bar is to be taken out, and afterwards replaced. The fresh coals, while the grate continues in the same position, will throw up as usual a body of thick smoke. But every one accustomed to coal fires in common grates must have observed, that pieces of fresh coal stuck in below among the red coals have their smoke so heated, as that it becomes flame as fast as it is produced, which flame rises among the coals and enlivens the appearance of the fire. Here then is the use of this swivel grate. By a push with your tongs or poker, you turn it on its pin till it faces the back of the chimney, then turn it over on its axis gently till it again faces the room, whereby all the fresh coals will be found under the live coals, and the greater part of the smoke arising from the fresh coals will, in its passage through the live ones, be heated so as to be converted into flame; whence you have much more heat from them, and your red coals are longer preserved from consuming. I conceive this construction, though not so complete a consumer of all the smoke as the vase, yet to be fitter for common use, and very advantageous. It gives too a full sight of the fire, always a pleasing object, which we have not in the other. It may with a touch be turned more or less from any one of the company, that desires to have less of its heat, or presented full to one just come out of the cold. And, supported in a horizontal position, a tea kettle may be boiled on it.

The author's description of his Pennsylvania fireplace, first published in 1744, having fallen into the

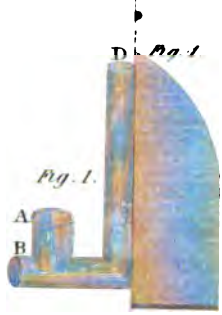


Fig. 1.

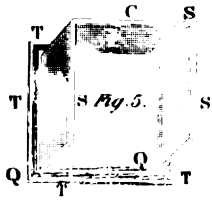


Fig. 5.

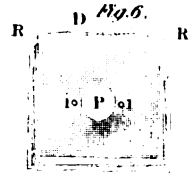
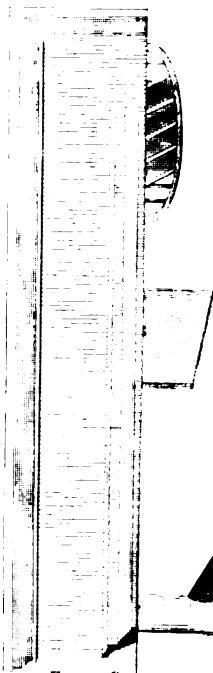


Fig. 6.

Fig. 7.



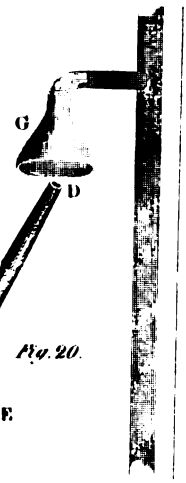


Fig. 20.

Fig. 9.

Fig. 10.

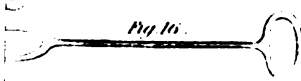


Fig. 16.

hands of workmen in Europe, who did not, it seems, well comprehend the principles of that machine, it was much disfigured in their imitations of it; and one of its main intentions, that of admitting a sufficient quantity of fresh air warmed in entering through the air-box, nearly defeated, by a pretended improvement, in lessening its passages to make more room for coals in a grate. On pretence of such improvements, they obtained patents for the invention, and for a while made great profit by the sale, till the public became sensible of that defect in the expected operation. If the same thing should be attempted with this vase stove, it will be well for the buyer to examine thoroughly such pretended improvements, lest, being the mere productions of ignorance, they diminish or defeat the advantages of the machine, and produce inconvenience and disappointment.

The method of burning smoke, by obliging it to descend through hot coals, may be of great use in heating the walls of a hot-house. In the common way, the horizontal passages or flues that are made to go and return in those walls, lose a great deal of their effect when they come to be foul with soot; for a thick blanket-like lining of soot prevents much of the hot air from touching and heating the brick work in its passage, so that more fire must be made as the flue grows fouler; but by burning the smoke they are kept always clean. The same method may also be of great advanvage to those businesses, in which large coppers or caldrons are to be heated.

Written at Sea, 1785.

Description of an Instrument for taking down Books from high Shelves.

January, 1786.

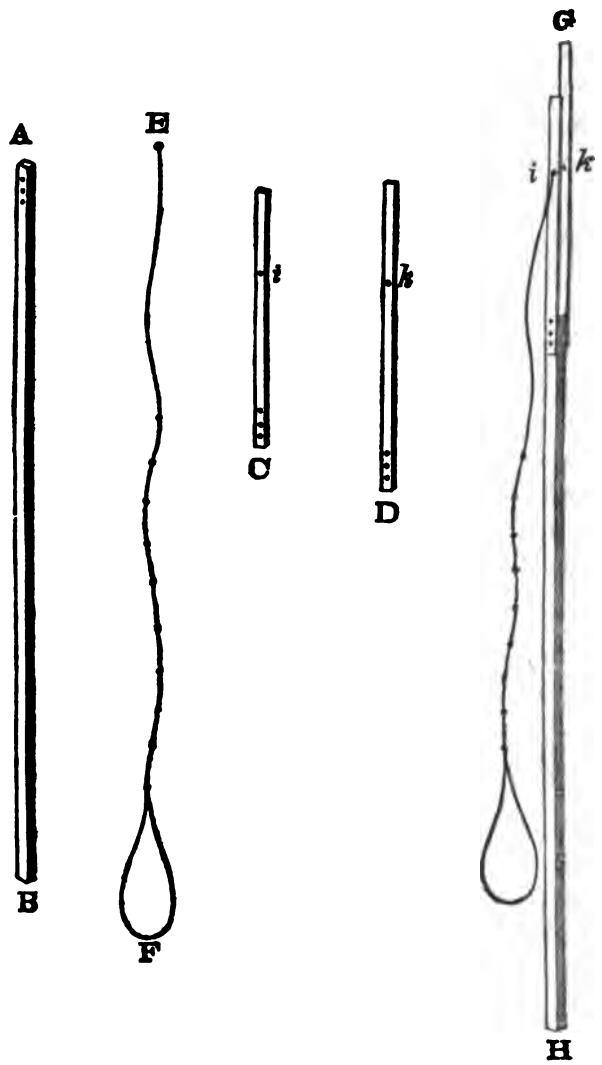
Old men find it inconvenient to mount a ladder or steps for that purpose, their heads being sometimes subject to giddinesses, and their activity, with the steadiness of their joints, being abated by age; besides the trouble of removing the steps every time a book is wanted from a different part of their library.

For a remedy, I have lately made the following simple machine, which I call the *Long Arm.*

A B, the *Arm*, is a stick of pine, an inch square and 8 feet long. *C, D*, the *Thumb* and *Finger*, are two pieces of ash lath, an inch and half wide, and a quarter of an inch thick. These are fixed by wood screws on opposite sides of the end *A* of the arm *A B;* the finger *D* being longer and standing out an inch and half farther than the thumb *C.* The outside of the ends of these laths are pared off sloping and thin, that they may more easily enter between books that stand together on a shelf. Two small holes are bored through them at *i, k. E F*, the sinew, is a cord of the size of a small goosequill, with a loop at one end. When applied to the machine it passes through the two laths, and is stopped by a knot in its other end behind the longest at *k.* The hole at *i* is nearer the end of the arm than that at *k*, about an inch. A number of knots are also on the cord, distant three or four inches from each other.

To use this instrument; put one hand into the loop, and draw the sinew straight down the side of the arm; then enter the end of the finger between the book you would take down and that which is next to it. The

laths being flexible, you may easily by a slight pressure sideways open them wider if the book is thick, or close them if it is thin by pulling the string, so as to enter



the shorter lath or thumb between your book and that which is next to its other side, then push till the back of your book comes to touch the string. Then draw

the string or sinew tight, which will cause the thumb and finger to pinch the book strongly, so that you may draw it out. As it leaves the other books, turn the instrument a *quarter* round, so that the book may lie flat and rest on its side upon the under lath or finger. The knots on the sinew will help you to keep it tight and close to the side of the arm as you take it down hand over hand, till the book comes to you; which would drop from between the thumb and finger if the sinew was let loose.

All new tools require some practice before we can become expert in the use of them. This requires very little.

Made in the proportions above given, it serves well for books in duodecimo or octavo. Quartos and folios are too heavy for it; but those are usually placed on the lower shelves within reach of hand.

The book taken down, may, when done with, be put up again into its place by the same machine.

TO BENJAMIN VAUGHAN.

On the Effects of Lead upon the Human Constitution.

Philadelphia, 31 July, 1786.

DEAR FRIEND,

I recollect, that, when I had last the pleasure of seeing you at Southampton, now a twelvemonth since, we had some conversation on the bad effects of lead taken inwardly; and that at your request I promised to send you in writing a particular account of several facts I then mentioned to you, of which you thought some good use might be made. I now sit down to fulfil that promise.

The first thing I remember of this kind was a general discourse in Boston, when I was a boy, of a complaint from North Carolina against New England rum, that it poisoned their people, giving them the dry bellyache, with a loss of the use of their limbs. The distilleries being examined on the occasion, it was found that several of them used leaden still-heads and worms, and the physicians were of opinion, that the mischief was occasioned by that use of lead. The legislature of Massachusetts thereupon passed an act, prohibiting, under severe penalties, the use of such still-heads and worms thereafter.

In 1724, being in London, I went to work in the printing-house of Mr. Palmer, Bartholomew Close, as a compositor. I there found a practice, I had never seen before, of drying a case of types (which are wet in distribution) by placing it sloping before the fire. I found this had the additional advantage, when the types were not only dried but heated, of being comfortable to the hands working over them in cold weather. I therefore sometimes heated my case when the types did not want drying. But an old workman, observing it, advised me not to do so, telling me I might lose the use of my hands by it, as two of our companions had nearly done, one of whom, that used to earn his guinea a week, could not then make more than ten shillings, and the other, who had the dangles, but seven and sixpence. This, with a kind of obscure pain, that I had sometimes felt, as it were in the bones of my hand when working over the types made very hot, induced me to omit the practice. But talking afterwards with Mr. James, a letter-founder in the same Close, and asking him if his people, who worked over the little furnaces of melted metal, were not subject to that disorder; he made light of any danger from the effluvia, but ascribed it to par-

ticles of the metal swallowed with their food by slovenly workmen, who went to their meals after handling the metal, without well washing their fingers, so that some of the metalline particles were taken off by their bread and eaten with it. This appeared to have some reason in it. But the pain I had experienced made me still afraid of those effluvia.

Being in Derbyshire at some of the furnaces for smelting of lead ore, I was told, that the smoke of those furnaces was pernicious to the neighbouring grass and other vegetables; but I do not recollect to have heard any thing of the effect of such vegetables eaten by animals. It may be well to make the inquiry.

In America I have often observed, that on the roofs of our shingled houses, where moss is apt to grow in northern exposures, if there be any thing on the roof painted with white lead, such as balusters, or frames of dormant windows, &c., there is constantly a streak on the shingles from such paint down to the eaves, on which no moss will grow, but the wood remains constantly clean and free from it. We seldom drink rainwater that falls on our houses; and if we did, perhaps the small quantity of lead, descending from such paint, might not be sufficient to produce any sensible ill effect on our bodies. But I have been told of a case in Europe, I forget the place, where a whole family was afflicted with what we call the dry bellyache, or *colica pictorum*, by drinking rain water. It was at a country-seat, which, being situated too high to have the advantage of a well, was supplied with water from a tank, which received the water from the leaded roofs. This had been drunk several years without mischief; but some young trees planted near the house growing up above the roof, and shedding their leaves upon it, it was supposed that an acid in those leaves had corroded the lead they covered,

and furnished the water of that year with its baneful particles and qualities.

When I was in Paris with Sir John Pringle in 1767, he visited *La Charité*, a hospital particularly famous for the cure of that malady, and brought from thence a pamphlet containing a list of the names of persons, specifying their professions or trades, who had been cured there. I had the curiosity to examine that list, and found that all the patients were of trades, that, some way or other, use or work in lead; such as plumbers, glaziers, painters, &c., excepting only two kinds, stonecutters and soldiers. In them I could not reconcile it to my notion, that lead was the cause of that disorder. But on my mentioning it to a physician of that hospital, he informed me that the stonecutters are continually using melted lead to fix the ends of iron balustrades in stone; and that the soldiers had been employed by painters, as laborers, in grinding of colors.

This, my dear friend, is all I can at present recollect on the subject. You will see by it, that the opinion of this mischievous effect from lead is at least above sixty years old; and you will observe with concern how long a useful truth may be known and exist, before it is generally received and practised on.

I am, ever, yours most affectionately,

B. FRANKLIN.

On Thermometers.

13 September, 1786.

THE two thermometers most generally in use at present, among the philosophers of Europe, are those of Réaumur and Fahrenheit. The French use Réaumur's, the English Fahrenheit's.

In their respective graduations, Réaumur marked his freezing point 0, Fahrenheit fixed his at 32 of his degrees above 0, and two of his degrees are just equal to one of Réaumur's. I know that in some instruments shis equality is not exact; but, in two which I have, the one Réaumur's, made by Cappy in Paris, the other Fahrenheit's, by Nairne, London, it is precisely so, they hanging together in the same room. And those workmen are famed for their exactness.

In reading, one frequently finds degrees of heat and cold mentioned, as measured by one or the other of those thermometers, and one is at a loss to reduce that least known to the other.

Rule.

Suppose the degree mentioned is 25 of Réaumur, which is 25 degrees above 0, or its freezing point, and you would know to what degree of Fahrenheit that answers;

Double the 25, which will give you 50 of Fahrenheit's, and to them add 32, his number at the freezing point, and you will have 82, the degree of Fahrenheit's equal to 25 of Réaumur.

On the contrary, if you would reduce Fahrenheit to Réaumur, first subtract 32, and then take half of the remainder; thus taking 32 from 82, there remains 50, and the half of 50 is 25.

This answers in all cases where the degree is above the freezing point.

If below, double the degrees of Réaumur, and subtract them from the 32 of Fahrenheit, which will give you the equivalent degree of his scale. Thus, suppose it 5 below 0, or the freezing point of Réaumur; twice 5 is 10, which deducted from 32, Fahrenheit's freezing

point, gives you 22 as the equivalent degree of his thermometer.

And halving the degrees of Fahrenheit that are less than 32, you have the degree of Réaumur. Thus 22 of Fahrenheit being 10 degrees less than 32, the half of 10 is 5, the equivalent degree of Réaumur.

B. FRANKLIN.

FROM WILLIAM HERSCHEL TO B. FRANKLIN.

With a Catalogue of new Nebulæ and Clusters of Stars.

Slough, near Windsor, 18 February, 1787.

SIR,

Give me leave to express my thanks to you as President in particular, and to all the Vice-Presidents, Secretaries, and members in general, for the honor conferred on me by electing me a member of the American Philosophical Society held at Philadelphia. As a small token of my gratitude I send hereby a catalogue of one thousand new nebulæ and clusters of stars; and at the same time communicate to the Society, that on the 11th of January, 1787, I discovered two Satellites revolving round the Georgian Planet; the first in about eight days and three quarters, and the second in about twelve and a half. The times of the revolution, and other circumstances concerning the orbits of these satellites, will be determined with greater accuracy hereafter. I detected them in consequence of an improvement in my twenty feet reflector, whereby I have gained much light. An account of this improvement is mentioned in a note at the end of the catalogue of nebulæ.

The acquaintance, Sir, I have long had with your literary character, makes me seize this opportunity with pleasure, of expressing my esteem for you, and of beg-

ging at the same time, that you will render my respects acceptable to all the members of the Society.

I have the honor to be, Sir,

Your most obedient humble servant,

WILLIAM HERSCHEL.

TO M. LE ROY.

On Balloons. — Pigeons killed by Lightning.

Philadelphia, 18 April, 1787.

MY DEAR OLD FRIEND,

I believe I have not written to you since I received your kind letters of July 26th, and October 9th, 1786. Such has been my continual occupation in public and private business, having the building of three houses upon my hands, that I had no time left for philosophical correspondence. I now take up my pen with the honest resolution of paying off some of my debts.

You mention, that M. de Buffon, *avoit des douleurs semblables aux miennes.* I sympathize with him. Let me know in your next how he does. I do not understand these dispensations of Providence, though probably they are for the best. But it seems to me, that, if you or I had the disposition of good and evil in this world, so excellent a man would not have an hour's pain during his existence.

Your account of the progress made in the art of ballooning, by the acquisition of a tight *enveloppe* and the means of descending and rising without throwing out ballast, or letting out air, is very pleasing. I am sorry the artists at Javelle do not continue their experiments. I always thought they were in the likeliest way of making improvements, as they were remote from interruption in their experiments. I have sometimes wished

I had brought with me from France a balloon sufficiently large to raise me from the ground. In my malady it would have been the most easy carriage for me, being led by a string held by a man walking on the ground. I should be glad to have Mr. Meunier's work. Pray let Mr. Grand know where he may buy it for me.

It gives me pleasure to hear of the success attending the conductors at Brest and at Dijon. Time will bring them more into use, and of course make them more useful.

It is a curious fact, that of the death of so many pigeons by lightning without disturbing their position. Pray when you see M. de Malesherbes, present to him my respects. He is one of the most respectable characters of this age.

Believe me ever, my dear friend, with the sincerest esteem and respect, yours most affectionately,

B. FRANKLIN.

FROM NEVIL MASKELYNE TO B. FRANKLIN.

Concerning Churchman's Theory of the Variation of the Magnetic Needle.

Greenwich, 3 March, 1788.

SIR,

On the 2d of May last year I received from you a paper concerning the variation of the compass, by Mr. Churchman of Philadelphia, of which you desired my opinion. As he at the same time sent another similar paper to the Board of Longitude, of which I am a member, I did not think I could properly send you my private opinion till that of the Board had been

taken. I have now the pleasure to acknowledge the favor of your letter, and to acquaint you, that the Board of Longitude considered it last Saturday, and agreed it was not new, the idea of accounting for the variation having been published in the Berlin Memoirs for 1757, from two poles not diametrically opposite, by the learned Mr. Leonard Euler, in a mathematical and masterly manner. The observations of variation at sea, owing to the iron work in the ship, and arms on board, are liable to great uncertainty, so that differences have been found of six degrees in the English Channel. There will be a great difference often, according as the ship is put on one or the other tack, owing to the soft iron on board becoming temporary magnets from the effect of the earth as a great magnet. Magnetic rocks at sea will disturb the magnet, and severe cold in northern regions seems occasionally to render it torpid, though it recovers itself again.

On all these accounts, and some others not less important, the variation of the compass cannot be considered as a general method of finding the longitude at sea, and is scarce of any use that way, now we have so much better methods of attaining the end. Mr. Churchman's supposition of a gradual change of the magnetic poles, without offering any probable physical hypothesis to account for it, must be considered as a mere hypothesis. You, Sir, who are so well able to judge of philosophical matters and physical causes, will have little doubt to join in opinion with the late Dr. Halley, as I do, that the gradual change of the magnetic poles cannot be probably accounted for from any gradual changes of the quantity, metallic state, magnetism, or translation of the iron and iron ore in the bowels of, or diffused through the surface of, the earth. Dr. Halley's hypothesis of four poles, two belonging to

an outer shell, and two to an inner nucleus, movable about the axis with a less velocity of rotation than the outer shell, is very ingenious and well calculated to get over this difficulty.

Observations both of the variation and dip of the needle, made throughout your continent, would be of use to throw light on this matter. Mr. Churchman might have been well satisfied with the judgments of such able men and good philosophers as Mr. Ewing and Mr. Rittenhouse. Mr. Dillwhynn sent me another of his proposals, with the disputes between him and the principal mathematicians with you, for the Royal Society, which I forwarded there.

I hope you receive (I mean your Philosophical Society) my Greenwich observations, now published up to the end of 1786, and published annually. They are ordered to you by the Council. I shall be gratified by the continuance of the present of your Memoirs, if thought proper, and am sensible of the honor of being a member. Your future correspondence will do honor to us.

Your most humble servant and old friend,

N. MASKELYNE.*

* Mr. Churchman supposed he had made valuable discoveries in the properties of the magnetic needle, by which its variation and dip might be ascertained for any given time and place; and also that he had discovered a new method of finding the longitude and explaining the theory of the tides. — EDITOR.

TO JAMES BOWDOIN.

Queries and Conjectures relating to Magnetism and the Theory of the Earth.

READ AT A MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY, JANUARY 15TH, 1790.

Philadelphia, 31 May, 1768.

DEAR SIR,

I received your favors by Messrs. Gore, Hilliard, and Lee, with whose conversation I was much pleased, and wished for more of it; but their stay with us was too short. Whenever you recommend any of your friends to me, you oblige me.

I want to know whether your Philosophical Society* received the second volume of our Transactions. I sent it, but never heard of its arriving. If it miscarried, I will send another. Has your Society among its books the French work *Sur les Arts et les Métiers?* It is voluminous, well executed, and may be useful in our country. I have bequeathed it them in my will; but if they have it already, I will substitute something else.

Our ancient correspondence used to have something philosophical in it. As you are now more free from public cares, and I expect to be so in a few months, why may we not resume that kind of correspondence? Our much regretted friend Winthrop once made me the compliment, that I was good at starting game for philosophers; let me try if I can start a little for you.

Has the question, how came the earth by its magnetism, ever been considered?

Is it likely that *iron ore* immediately existed when this globe was first formed; or may it not rather be supposed a gradual production of time?

* The American Academy of Arts and Sciences. — EDITOR.

If the earth is at present magnetical in virtue of the masses of iron ore contained in it, might not some ages pass before it had magnetic polarity?

Since iron ore may exist without that polarity, and by being placed in certain circumstances may obtain it from an external cause, is it not possible that the earth received its magnetism from some such cause?

In short, may not a magnetic power exist throughout our system, perhaps through all systems, so that if men could make a voyage in the starry regions, a compass might be of use? And may not such universal magnetism, with its uniform direction, be serviceable in keeping the diurnal revolution of a planet more steady to the same axis?

Lastly, as the poles of magnets may be changed by the presence of stronger magnets, might not, in ancient times, the near passing of some large comet, of greater magnetic power than this globe of ours, have been a means of changing its poles, and thereby wrecking and deranging its surface, placing in different regions the effect of centrifugal force, so as to raise the waters of the sea in some, while they were depressed in others?

Let me add another question or two, not relating indeed to magnetism, but, however, to the theory of the earth.

Is not the finding of great quantities of shells and bones of animals (natural to hot climates) in the cold ones of our present world, some proof that its poles have been changed? Is not the supposition, that the poles have been changed, the easiest way of accounting for the deluge, by getting rid of the old difficulty how to dispose of its waters after it was over? Since, if the poles were again to be changed, and placed in the present equator, the sea would fall there about fifteen miles in height, and rise as much in the present polar

regions; and the effect would be proportionable, if the new poles were placed anywhere between the present and the equator.

Does not the apparent wreck of the surface of this globe, thrown up into long ridges of mountains, with strata in various positions, make it probable, that its internal mass is a fluid; but a fluid so dense as to float the heaviest of our substances? Do we know the limit of condensation air is capable of? Supposing it to grow denser *within* the surface, in the same proportion nearly as it does *without*, at what depth may it be equal in density with gold?

Can we easily conceive how the strata of the earth could have been so deranged, if it had not been a mere shell supported by a heavier fluid? Would not such a supposed internal fluid globe be immediately sensible of a change in the situation of the earth's axis, alter its form, and thereby burst the shell, and throw up parts of it above the rest? As, if we would alter the position of the fluid contained in the shell of an egg, and place its longest diameter where the shortest now is, the shell must break; but would be much harder to break, if the whole internal substance were as solid and hard as the shell.

Might not a wave, by any means raised in this supposed internal ocean of extremely dense fluid, raise in some degree, as it passes, the present shell of incumbent earth, and break it in some places, as in earthquakes? And may not the progress of such wave, and the disorders it occasions among the solids of the shell, account for the rumbling sound being first heard at a distance, augmenting as it approaches, and gradually dying away as it proceeds? A circumstance observed by the inhabitants of South America in their last great earthquake, that noise coming from a place

some degrees north of Lima, and being traced by inquiry quite down to Buenos Ayres, proceeded regularly from north to south at the rate of leagues per minute, as I was informed by a very ingenious Peruvian whom I met with at Paris.

B. FRANKLIN.

Description of the Process to be observed in making large Sheets of Paper in the Chinese Manner, with one smooth Surface.

IN Europe, to have a large surface of paper connected together, and smooth on one side, the following operations are performed.

1. A number of small sheets are to be made separately.

2. These are to be couched, one by one, between blankets.

3. When a heap is formed it must be put under a strong press, to force out the water.

4. Then the blankets are to be taken away, one by one, and the sheets hung up to dry.

5. When dry, they are to be again pressed, or, if to be sized, they must be dipped into size made of warm water, in which glue and alum are dissolved.

6. They must then be pressed again to force out the superfluous size.

7. They must then be hung up a second time to dry, which, if the air happens to be damp, requires some days.

8. They must then be taken down, laid together, and again pressed.

9. They must be pasted together at their edges.

10. The whole must be glazed by labor, with a flint.

In China, if they would make sheets, suppose of four and a half ells long and one and a half ells wide, they